

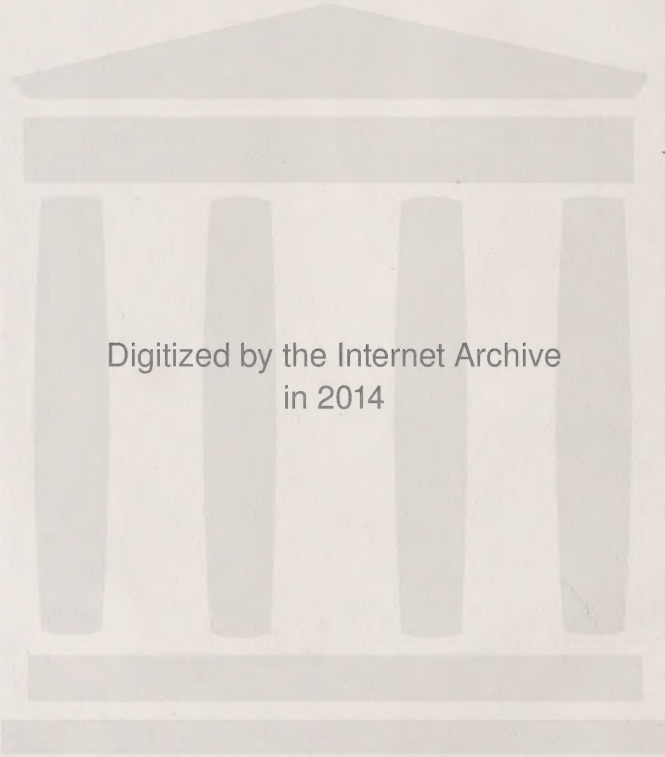
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NEW ORLEANS

Medical News and Hospital Gazette:

A MONTHLY JOURNAL,

EDITED BY

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NEW ORLEANS

MEDICAL NEWS AND HOSPITAL GAZETTE.

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VOL. II.

MARCH 1, 1855.

NO. I.

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Proceedings of the State Medical Society of Louisiana,  
ASSEMBLED IN THE UNIVERSITY BUILDINGS, FEB. 8, 1855.

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*The Yellow Fever at Natchez in all its bearings on the Quarantine question and Yellow Fever at New Orleans.*

---

DR. SAMUEL A. CARTWRIGHT made the following Report:

*Gentlemen:*—In obedience to your Resolution of the 6th instant, calling on me for a Report on “*The Yellow Fever of Natchez in all its bearings on the Quarantine question, and the Yellow Fever at New Orleans,*” I beg leave to say that I do not intend to discuss the question of quarantine or no quarantine; but only to call attention to those facts which relate to the most rational and effective kind of Quarantine. Although a large number of our citizens and a majority of the profession may believe that it is impossible to prevent yellow fever by any quarantine regulations whatever, and that it is safer for the public health to let trade be free than to shackle it, yet I have assumed that public sentiment demands a quarantine and that it is useless to resist it. I therefore propose to call attention only to those facts that may enable our governing authorities to see their way more clearly in establishing an effective quarantine, or at least, one the least likely to disappoint the hopes of its advocates. Those, who believe in the utility of such a measure and in the communicability or contagiousness of yellow fever, differ greatly among themselves in regard to the nature of contagion and the best measures to prevent its introduction and the spread of yellow fever when it does appear, whether imported or

not. The doctrine that one party holds, is, that the disease was originally imported from Boulam, in Africa, or from Siam, in Asia, and in all instances, propagated by contagion and by contagion alone; that the contagion is not only transmitted from person to person but adheres to goods and merchandize; that it can exist for weeks, months, and years, and does exist in all boxes, bales and packages of goods put up in, or passing through infected districts, and can propagate the disease wherever these goods are sent, received and opened. It is evident that nothing short of absolute non-intercourse can insure safety from such a contagion as that. A quarantine, the most rigid and protracted, and the most thorough fumigation and ventilation could be of no avail against such a contagion — supposed to adhere so closely to the ultimate particles of many species of merchandize, as raw cotton, woollens, silks and furs for instance, that no cleansing or fumigating process can ever reach or destroy without destroying the texture of the substances containing it. The British writers, of the last century, contended that what they called the genuine or Boulam yellow fever was propagated by a contagion of this kind. Dr. Chisholm gave instances where he supposed it adhered in walls of houses for years after a thorough washing, scrubbing, and ventilation — (and of course could adhere to goods as closely as to walls.) He also contended that there were two yellow fevers — a non-contagious, indigenous yellow fever of the West India Islands and a highly contagious Boulam yellow fever, which had got into the United States, viz: Boston, New York, Philadelphia, Baltimore, Alexandria, and Charleston, and intimated that the lawless republicans could never get it out. Spain got alarmed at the frightful accounts of the contagious or Boulam yellow fever prevailing in the United States, and instituted the most stringent quarantine regulations against its introduction into that kingdom; so stringent, that commerce was in a good measure destroyed, not only between Spain and America, but between one Spanish city or province and another, each suspecting the other of having the contagion, and in a short time lost a million of inhabitants; not from contagion, but from disease produced by the stagnation of commerce and the want and wretchedness created thereby. The wise men of Spain saw the impossibility of freeing American cotton from such a subtle, tenacious contagion by any other process than by fire. Hence, by law, ship and cargo, containing any American cotton on board, were burnt. Now, it so happened when the doctrine of there being two yellow fevers, a Boulam contagious one in the United States, and an indigenous one in Cuba and other West India Islands, was first propagated by Dr. Chisholm, an officer in the pay

•



of the British Government, that Great Britain and Spain were running a race in the manufacture of fine goods—Spain was ahead. All the good cotton used in the finer fabrics came from America, chiefly then from the sea islands of South Carolina. Instead of spinning, Spain burnt it, so did Denmark. The latter would not trust it to be burnt in Denmark, but ordered the vessels containing American cotton to be burnt on the coast of Norway. The British cotton manufacturers, more venturous and less afraid of contagion, soon got ahead of both those powers in manufacturing cotton goods. Such is a brief history of the doctrine, as preached and practiced by one of the parties holding to the contagiousness of yellow fever and the necessity of quarantine.

The other party also contended for the contagious nature of yellow fever, but attributed it to a contagion of a very different kind. They believed that the yellow fever of the United States and the West Indies was one and the same; that there was in truth but one; and that like typhus fever, it was contagious or non-contagious according to circumstances—contagious or infectious, in a close, damp, hot and impure atmosphere; and non-contagious in a clean, dry and well-ventilated apartment, or in the open air. Contagious, in one season or in some particular localities, even without filth, from some peculiar unknown state of the atmosphere; and non-contagious in another season, in another locality, even in the midst of filth and impurity; that exposure to the open air, for a few hours, would kill the contagion or infection in all goods and merchandize from an infected district, if the goods themselves were in a sound state, and were not damaged; whereas no ventilation or purification could ever destroy the contagious or noxious quality of putrescent or fungoid matter, come from where it would—soiled garments, for instance, shut up in trunks or bales, until they had become fungoid. These different doctrines, in regard to contagion, would necessarily lead to differences in the quarantine regulations that might be instituted to prevent the introduction of yellow fever, or its extension after it commenced, whether its origin were foreign or domestic. Without pretending to decide the question which of these two doctrines of contagion be the true one, (assuming that yellow fever is actually contagious or infectious, which many will not admit,) I simply wish to confine myself to removing some errors, misrepresentations and misstatements of matter of fact out of the way, in order that every one may be better able to judge for himself. Now, it has so happened, that both of the above mentioned doctrines, as to the nature of the contagion of yellow fever, have become confused and jumbled together in so intricate a way as to be very difficult to disentangle,

even by professional men of the most extended reading and the most enlarged experience. It is impossible for the public to untangle them, or the wisest legislators, without the direct aid of the medical profession. It is highly important that a clear line of distinction should be drawn between the two, because a quarantine, directed exclusively against the one, would rather tend to introduce and spread the other. Although the doctrine of the non-contagionists does not come within the line of inquiry I am pursuing, yet the causes, which have led so many enlightened physicians, and a large portion of our population to regard the yellow fever as non-contagious, may be very properly taken into consideration in determining the question, which of the two parties or schools of contagion is nearest to the truth? If yellow fever were propagated by a peculiar and specific contagion from Boulam or Siam, and in no other manner, there never would have been any non-contagionists at all. No one denies the contagious nature of small-pox. But, if it be like typhus fever, contagious or not according to circumstances of season, climate, local causes, atmospheric influences and bodily predisposition, then we can readily perceive why there should be so many non-contagionists. Observers in different times and places would form opinions based on what they saw, and these opinions would necessarily be conflicting. Thus, observers in one place, perceiving yellow fever begin in, and spread through some filthy locality, confining its ravages mostly thereto, would be apt to embrace what is called the filth theory; while others, perceiving it sometimes to break out in localities apparently cleanly, might conclude that filth had no agency in its march or causation whatever. Seeing some districts on the eastern side of the American continent afflicted with the disease, while others on the western side, much more filthy, entirely exempt from its ravages, some have concluded that filth has a preventive influence. Nor is it all unlikely that under certain circumstances it has that influence when strong enough, by reason of its ammoniacal salts to generate an atmosphere of its own kind—walling out, as it were, the surrounding atmospheric influence predisposing to the disease. But if not sufficiently ammoniacal for this purpose, there are abundant facts to show that it combines with and gives efficacy to the essential and unknown causes of the disease—one of which, for argument sake, we suppose to be contagion. When these causes are weak, some degree of atmospheric pollution is essential to the manifestation of their effects. But when strong and favored by some peculiar meteorological condition of the atmosphere, they can manifest their effects in certain seasons and localities upon highly sus-

ceptible and unacclimated persons without, or at least with very little local aid, from putrescent matter. The abnegation of contagion entirely in typhus fever, as well as that of giving undue weight and importance to it, have both, when reduced to practice, been attended with disastrous consequences. Neither those who contended that contagion was nothing, nor those who contended it was everything, *could prevent or arrest typhus*. It was found that the utmost cleanliness on ship-board could scarcely be depended upon to prevent that form of typhus, called ship fever, from occurring on board a ship crowded with passengers, if any individual from a jail or hospital, where typhus was prevailing, were admitted on board with his unwashed clothing. On the other hand, it was discovered that the most rigid and stringent regulations against contagion could not keep it out of large armies, besieged cities or any other places containing a dense population living in filth and poverty. The Allies, with the Czar to help them, cannot keep it out of the armies in and about Sevastopol. It was also discovered that ventilation, washing and scrubbing, would generally kill the contagion, or at least render it so innocuous that persons in London, living next door to typhus fever, need not fly, as they had been in the habit of doing, but had nothing to do but to keep their houses clean and well ventilated, in order to avoid it. Thus proving that if typhus be contagious, the contagion can act only at a very small distance, and does not contaminate the fresh open air. In a well ventilated hospital, none but the auscultators are apt to take the disease from typhus fever patients—as Professor Jones' case illustrates. There are abundant facts to prove that the contagious element in yellow fever, or the American typhus, as it is sometimes called, is nothing more than the contagious element in the European typhus, *if so much*, and is governed by the same laws and is easily counteracted or destroyed. As sanitary measures, founded upon the one idea of the contagiousness of the European typhus, after having been preserveringly tried for centuries, failed to prevent or arrest it, and as such measures, founded on the same exclusive idea, after having been for many years fairly and perseveringly tried in Spain to prevent and arrest the yellow fever, most signally failed, not only there, but elsewhere, it is but reasonable to conclude that they would fail again. The quaratines, heretofore established in Louisiana, failed, and it becomes us now to examine the subject more closely under the lights of reason and experience, and establish one which will not fail. But unless the true ground to build on be found, the whole will end in vast expense and disappointment. Now, there is both an inside and outside pressure operating upon our governing authorities, which has a

tendency to confuse and mislead them from the only true basis on which beneficial quarantine regulations can be founded. Nothing has a greater tendency to do so, than pressing upon the attention of the Legislature the doctrine of the African origin of yellow fever, as has lately been done by two individuals in an adjoining State, of great wealth, influence and high social position, not only over their own signatures in the newspapers, to influence public opinion, but with the avowed design of operating upon the Legislature of Louisiana now in session. These individuals are, Drs. J. C. Jenkins and Stephen Duncan. They are men who would not misstate facts if they knew it, or tell a part of the truth to mislead, if in possession of the whole truth. This, however, only gives their opinions, and the facts on which they are founded, the more weight and influence. As I am almost the only surviving physician who was an actor in the events of which they mostly treat, and witnessed the whole of many of the facts set forth in their publications, known to them only *in part*, the same motive—the public good—that impelled them to give the part that they know, impels me to present to this Society the other part, which they did not know. A mere fact is not truth, and is very apt to lead to error. It takes all the facts to corner truth, which is perpetually flying from isolated or one-sided *ex parte* facts.

Dr. Duncan accuses New Orleans of giving the yellow fever to Natchez in the year 1823. He also accuses Africa of giving it to America. He says that in 1823 he lived at Homochitto. Yet he "*Knows the yellow fever was in New Orleans in the early part of August.*" If he knew it, it must have been by information, and his informant must have missed a year and meant the year 1822. The records of the Charity Hospital show that 239 deaths occurred from yellow fever, in the year 1822, when there was no yellow fever in Natchez, and only one, *barely one death* from yellow fever in 1823, the very year that Natchez was decimated, and more severely scourged with the disease than she has been before or since. The death, that did occur, took place on the 11th of September, after the worst was over in Natchez. The only other case that occurred in the Charity Hospital and which recovered, was entered on the 23d of August, and curious enough, we learn from a note appended to this case by Dr. Kerr, and published by Dr. Dowler, that it was taken out of a flatboat lying at the mouth of Red River, and brought to this city by the steamboat Eagle. It came, therefore, no great way from Dr. Duncan's Homochitto plantation.



There is positive evidence that the first case in Charity Hospital, New Orleans, came down the river, and may have come from Homochitto, or what is more likely, from Natchez. Certainly the history of the epidemic of Natchez in 1823 proves most conclusively that none of the first cases that occurred in Natchez came up the river. Nor did the epidemic begin or spread from the side of the city next to the river, but from the other side, next to Dr. Duncan's Homochitto plantation, and on the street leading to the Homochitto road. It is not denied that there may have been in New Orleans some straggling cases of yellow fever, or disease called by that name in private practice, as there is every year, and of these Dr. Duncan may have heard, but he will evidently be not a little surprised to find that the books of the Charity Hospital and the authority of Doctors Fenner, Dowler, Barton and Simonds are all against there having been more than two cases and one death in that institution from yellow fever in the year 1823—and that both those cases occurred after the epidemic broke out in Natchez. As if, however, Dr. Duncan's effort to trace the origin of the disease to New Orleans might not be satisfactory, Dr. Jenkins, in the *Concordia Intelligencer* of the 26th ultimo, (January) in the same paper in which Dr. Duncan's letter is published, states on hear-say evidence, that the first case in Natchez in 1823, was a woman who had contracted the infection in the West Indies. But he did not know that Dr. Henry Tooley, an old resident of that city, living there at the time, had heard the same rumor and probed it to the bottom. It turned out, that the woman referred to, had never been in the West Indies, but came direct from London to New Orleans, without touching at any port on the way. But if she had come from the West Indies, as she arrived in Natchez in a healthy condition and did not sicken for a month or two afterwards, it is evident that the most stringent quarantine directed against the contagion of the Boulam yellow fever would not have prevented its introduction, unless it excluded all the young and healthy women. Dr. Jenkins quotes the high authority of Dr. James Metcalf to prove that the first case of yellow fever that he had in the year 1823, had not been near the locale that I pointed out as the place where the disease commenced. This is admitted, but as no day or date of the case is given it amounts to nothing, as the presumption is against its being among the first cases; because Dr. James Metcalf resided, in the year 1823, *not in the city of Natchez, where Dr. Jenkins locates him*, but twelve miles from the city, in the country. Dr. Jenkins is very complimentary in speaking of my history of the

yellow fever of Natchez in 1823, for which I thank him, yet he blames me with seeming to be ignorant of the fact that the yellow fever was prevailing in New Orleans in 1823. Doctors Barton, Dowler, and Simonds, and Dr. Fenner, who has taken much pains to gather all the facts in regard to the yellow fever of New Orleans, are also ignorant of its presence here in the year 1823, and the books of the Charity Hospital ignore it, except the two cases previously mentioned. Dr. Jenkins says: "As the question was one of interest and importance I made diligent inquiry, and have yet to meet the first man who has not informed me that the *cause* assigned by Dr. Cartwright for the epidemic was preposterous and absurd." Here is the history of the epidemic, written by me, of which Dr. Jenkins speaks. [Dr. C. here produced the book and handed it to the Secretary of the Society.] Let any one read it through and through, and he will perceive that I assigned *no cause* whatever for the epidemic, for the plain reason that I did not know, nor do I now. I gave a history of the facts attending its rise and progress, and gave no opinion whatever as to its *cause*. The error lies in Dr. Jenkins's informants supposing or taking it for granted that I had assigned *some cause*, and then at a venture condemning the same as "preposterous and absurd." According to the map which accompanies the essay, the first cases are located in and around a house containing a large quantity of putrid bacon, pork and fish, and some other cases in the locality of putrid oysters, but I did not say that these putrid substances *caused it*. Whatever the essential cause then existing in the locality of Natchez may have been, whether contagion or something else, observations were wanting to show whether putrescent substances gave efficacy to the essential cause or not. The history of the yellow fever of Natchez in 1823 and 1825 showed that they did. It may very properly be considered as preposterous and absurd to regard putrid matter as necessarily causing yellow fever—because facts prove that such putrid substances may and often do exist without producing such an effect. But when in a locality pervaded by an unknown influence that is the essential cause of yellow fever, facts rigidly observed, were wanting to show whether putridity gave activity to the secret, morbid influence within the infected district. And, again, facts were wanting to solve the question, whether ventilation and cleanliness, within and around the infected district, would not have the effect of making the contagion harmless, if it were contagious, or of neutralizing, to a greater or less extent, the poison or unknown agent that is the essential cause of the disease. Having gathered a number of facts on this important question by diligent observation—facts no less important,

whether yellow fever be caused by contagion or anything else, I committed them to writing in the shape of historical essays. Having done so, in order to have them corrected, they were submitted to a number of professional friends; when trimmed of errors as much as possible, the two Essays on the Yellow Fever of Natchez in 1823 and 1825, including the epidemic of the town of Washington, in the last named year, were submitted to the City Council and Board of Health of Natchez for examination, with the request, that if they found the statements therein contained to be correct, that they, who were cognizant of all or most of the facts and circumstances mentioned, would verify the same, as such a verification might be useful in after times, as being the best evidence that could be had of their truth, if it should ever hereafter be questioned. The city authorities, on motion of Dr. S. Gustine, complied with the request and passed a preamble and resolutions to the effect that they had read the essays, examined the facts, and found them to be substantially correct. They recorded the same, and furnished me with a copy, and here is the document. [Here Dr. Cartwright handed it to the Secretary of the Society to examine.] This document is not introduced to refute anything that Drs. Jenkins, or Duncan, or any one else may have said or heard; but to show to the Society the pains that were taken to arrive at and substantiate truth on a subject of great and momentous importance to the people of Louisiana at the present day, viz: whether filth and putrescent substances within a district, where the secret cause of yellow fever lurks, be that cause contagion or anything else, will make it more active; also, whether ventilation and cleanliness do not tend to make it less noxious or entirely harmless? On the latter of these questions I am now about to introduce to this Society a reverend divine, and to put him in a large, splendid house, well constructed for ventilation, as also the out-houses which were large and numerous. The shade-trees of the grounds around are not so thick as to cause too much dampness, or to obstruct a free circulation of air; yet sufficiently so to protect the surrounding premises against excessive solar heat. But I must bid time roll backwards thirty-two years, and show this house within the radius of the infected district of Natchez in the year 1823—it being about a mile in a direct line from the focus of the infection, and about a mile and a half by the road. The epidemic influence manifested itself to a greater distance from the focus than this house stands. That house is now owned and occupied as a family residence by Dr. Stephen Duncan; but, thirty-two years ago, it was occupied by a clergyman and

his family, none of whom had ever had the yellow fever. The clergyman, like the present occupant of the house, believed the yellow fever to be contagious; but he believed that fresh air, cleanliness, temperance, and whole heart devotion to the suffering sick had the same power over the yellow fever contagion that an observance of the ten commandments has over the devil, viz: destroys its power for evil by some law or kind provision of Providence, not well understood either by the divine or the physician. But that such a law exists, making it less dangerous to approach the sufferers in the plague, give them fresh air, make them clean, nurse them, and administer to their wants, than to fly from them, or shut the doors of the heart against them, there can be no doubt. Witness the Howard Association of this city. How wonderfully they have been preserved, as also those every where who devote themselves to the sick. When the yellow fever of 1823, broke out in Natchez, that clergyman threw wide open the doors of his house and out-houses to the inhabitants of the afflicted city, to the stranger as well as to the acquaintance — to the sick and the well — to the sinner and the pious; all alike were welcomed, and all served with every thing the house and premises could afford. The sick were nursed and waited on by the good pastor himself, his angel wife, his sons, his daughters, and his servants, without money and without price, as if they had been his own children. The house was full when an individual came riding from the city like a wounded man from the battle field. The yellow fever had struck him. He entered the house and fell exhausted on the first bed he could reach. It was the bed of one of the good parson's sons.

The parson would not let the patient leave the bed, but prepared one for his son in the same room. After a hard struggle, the patient, by kind attentions and good nursing, recovered. In less than two weeks he returned to the city and resumed the practice of medicine. He is your present speaker. But let me read from the medical recorder, vol. 9, page 9 and 10 — Phil., 1826. "The Rev. Mr. Burruss lived only a mile and a half from Natchez. Two families besides his own, together with several young men, resided with him, and several other families occupied the out-houses. Persons with yellow fever were taken in, nursed and attended to by the family; yet no one took the disease. Some of them were also present and afforded me assistance in the dissection of several subjects, who died of the yellow fever. The dissection was carried on in an out-house, but a few rods from the house occupied by the three families that escaped the disease." None of the individuals here alluded to had ever had the yellow fever. I



do not quote the above extract to prove its non-contagiousness, but only to prove that, if it be contagious, the contagion is dead, inanimate, inoperative, under proper ventilation and cleanliness, and when the heart, instead of being shrunken and frozen with fear, is warmed and expanded by that noblest impulse in man's nature—devotion to the cause of suffering humanity. But I will not vouch for its non-contagiousness in an ill-ventilated, impure atmosphere—especially, if terror be permitted to chill the blood in the veins by banishing benevolence and its warmth from the human breast.

The Rev. gentleman mentioned in the extract just read from the Medical Recorder, of 1826, was the Rev. John C. Burruss, father-in-law of that *rara avis*—a popular and pious *millionaire*—Judge McGehee of Wilkinson County, Mississippi. The house mentioned in the extract was the very house now occupied by Dr. Stephen Duncan as a residence. I was the person who made the dissections. The out-house spoken of as the place where the dissections were made, was the hen-house—quite a large and showy building. One evening wishing to examine the brain of a man who had died with yellow fever in the Natchez Hospital, and being called in haste to the parson's, I cut off the head and tied it up snugly in a handkerchief, and dropped in the parson's, now Dr. Duncan's hen-house, intending to return and examine it at my leisure, but was called back to the city and did not return until next day. On searching the hen-house the head and handkerchief were no where to be found, and the parson missed two of his fattest turkeys. Some days afterwards the head was found on the top of a hill, at the edge of the woods. The handkerchief was untied, and it was sitting upright on the neck, with the eyes wide open. Two turkey gobblers fastened together were lying close by it, and tracks fifteen feet apart down the hill were plainly impressed upon the yielding soil. After this the parson troubled himself no further with locking the hen-house doors, and never lost a chicken or turkey by the thieves. It is said that locks are unnecessary to that hen-house, even down to the present day.

Vast as Dr. Duncan's wealth is, that good parson whom I have introduced to this Society is richer, although he might be mistaken for a poor man, at the present time, on his cotton plantation in the Parish of Caddo. Dr. Duncan is very generous, and gives large sums to charitable purposes; but the parson gave not only his wealth, but his hands and heart to his suffering fellow-men, and set an example in dealing with the contagion of yellow fever that need only be followed to destroy it, and make the South blossom as the rose. Whereas, if Dr. Duncan's theory of dealing with con-

tagion be followed, the South will be blighted with desolation, as Spain was by practicing on the same theory. What is that theory? It has gone forth to the public through the newspapers, to be put in the hands of members of the Legislature, and is well calculated to mislead the public as well as our law-makers on a subject of vital interest. The reason given by Dr. Duncan for changing his views on the subject of contagion, however valid and weighty with non-professional readers, will have no more than its due weight with the members of the Louisiana State Medical Society, the Professors of the University, and the Editors of the Medical Journals, here assembled in the hall of the Medical College. Dr. Duncan informs us that at Saratoga he met with a Dr. Stevens who told him that he had seen an old woman of Bristol, and that the woman told him, she had lost two daughters with the yellow fever, who had opened the trunk and washed the clothes of a brother that died at sea with the same disease. Dr. Stevens was also told that the same young man was the mate of the brig from Laguira, which had landed the damaged hides and coffee at the Walnut street wharf in Philadelphia in 1820—the very cargo which Dr. Duncan, with his old notions about yellow fever, had supposed to have been the cause of the yellow fever showing itself on the Walnut street wharf in the vicinity of damaged hides and coffee. But on hearing Dr. Stevens's story, he changed his mind and no longer blamed the damaged cargo with the mischief, but contagion—the African yellow fever contagion—the same hobgoblin that traveled on the trunk up to Bristol and killed the old woman's daughters. Dr. Duncan's quarantine, therefore, would let in damaged hides and coffee, if coming from a port where there was no contagion, but he would exclude boxes of merchandize, however sound, coming from a district where the yellow fever prevailed. Such a quarantine would not be effectual, as it would be directed against the shadow and not against the substance. The facts related by Dr. Duncan, although on hear-say evidence, are not doubted at all, because similar facts abound in medical history. But non-professional persons might suppose that such circumstances were unknown to the medical faculty, and they would be apt to jump to the conclusion, that if contagion was a thing which could ride on a trunk all the way across the sea, and up the Delaware to Bristol, it could ride the same distance on any other box, bale or package of merchandize, however sound and well ventilated. Dr. Rush was in the habit of telling a similar trunk story, but he explained it by the bad air and noxious matter of the old musty soiled and damaged clothing contained in the trunk. The further such a trunk went, and the longer its detention at the quarantine ground, the worse it would get. Whereas the first breath of pure air would wash all the con-

tagion off a hogshead of sugar or barrel of molasses from an infected district, or any other barrel, box or package of sound merchandize. Dr. Duncan's theory would treat the sound and unsound alike, on the supposition that a winged messenger of destruction rode upon every package of merchandize from an infected district.

The Spanish quarantine often detained sound merchandize at the quarantine ground, until it became unsound, and then let it in. It failed, as every other quarantine will, which is not founded on rational principles. Both Drs. Jenkins and Duncan seem to attribute marvelous virtues to the Natchez quarantine, which by-the-bye was no quarantine at all, strictly speaking. During the last thirty-eight years, Natchez has suffered with the yellow fever eleven times, besides a sporadic case or two almost every year. Five of those visitations occurred during the eighteen years when there was no quarantine; and the other six during the twenty years of quarantine. In 1817 the first epidemic occurred—no quarantine. In 1819 another severe epidemic happened in the face of the strictest quarantine that Natchez ever had. It was rigid in the extreme, even to fumigating letters that passed through the post office. The dreadful epidemic of 1823 occurred under the quarantine regimen. When the epidemic of 1825 made its appearance, the quarantine mania had nearly exhausted itself. So much so, that John Orr, the first case, (12th August) who occupied a warehouse containing damaged porter and pork, in that part of the town called under the Hill, was taken On the Hill in the midst of the city, and nursed and attended to until he died with black vomit. During August and September, Natchez On the Hill, notwithstanding the introduction of yellow fever patients from under the Hill, continued remarkably healthy. On the 18th September, it broke out in Washington, a town six miles east of Natchez, where it had never occurred before, although in 1823, it had opened its doors to the sick and dying from Natchez.

But in 1825 many of the sojourners, and some of the citizens of Washington fled to Natchez On the Hill to avoid the pestilence. About the 1st of October, it broke out almost simultaneously in three different places On the Hill. Such is a short history of the vagaries of the yellow fever of Natchez in 1825, which seemed to have made a complete revolution in public opinion in regard to quarantine; and in ten years, from 1825 to 1836, there was no quarantine or any mention or agitation of the subject. During these ten years, there was no yellow fever, except a few cases in 1829. During these ten years (see Fenner's Southern Reports, and Dr. Simonds' Tables,) the yellow fever was in New Orleans every year, and seven years

out of the ten, prevailed quite extensively, the smallest number of deaths from it in the Charity Hospital—any one of the seven years—being ninety-five. So far from their being a quarantine in Natchez, the citizens followed the example of the good parson, whom I have mentioned, threw wide open their doors to the sick and the dying from New Orleans—took them out of the steamboats and carried them to their houses—nursed and attended to them until they died with black vomit or recovered—and not a case of yellow fever occurred during the whole period of ten years among the citizens, that I could hear of, except a few cases in 1829. At length the good parson left the country—a new generation grew up that “knew not Joseph.” The years 1837 and 1839 were signalized by epidemics and panics—the good parson’s example being almost entirely forgotten. After this the subject of quarantine began to be agitated, and from 1843 to 1853, inclusive, there was a quarantine. Four years out of the ten, the yellow fever leaped the quarantine and attacked the inhabitants, viz: in 1844, 1847, 1848 and 1853. That of 1853 there is no dispute about. Dr. Stone, of Natchez, published an account of the epidemic yellow fever of Natchez in 1848. I lost a promising student that year, Mr. Price, with the black vomit, who had not been out of the city. Accounts of the yellow fever of 1844 and 1847 have been collected and published by Dr. Fenner, an active member of this Society, and who has bestowed much time and attention upon the subject. Dr. Craig died with black vomit in Natchez in 1844. (See Dr. James Young’s cases that year, published in the *New Orleans Medical and Surgical Journal*, vol. vii., p. 226.) In the year 1844, New Orleans almost entirely escaped, yet Woodville was desolated. That year I was called to see a lady at the Mansion House full of boarders. The moment I entered the room, the eyes of the patient glaring like a wild beast in its lair, told me that it was a case of yellow fever. She implored me not to tell she was from Woodville and had the fever, for fear she would be turned out of the house. She had been attacked on the way in the stage-coach. But it soon became known, not through me, however. There were many lady boarders in the house, and I promised them that I would insure them against catching the disease, if they would nurse the poor stranger, and keep the doors and windows open—that such goodness would banish fear and guard them against the infection by some law of our nature not well understood. They did so. All escaped the disease, except one, who never came near the patient, but locked herself up in a room in the most distant corner of the house. Her physician was Dr. Lloyd, and his diagnosis was yellow fever.



The object of Drs. Jenkins and Duncan, in their publications, clearly appears to be to impress on the public in general, and the Louisiana Legislature in particular, that a quarantine has protected Natchez; ergo that New Orleans ought to have a similar quarantine to protect herself and the South. The error lies in the assumption that the quarantine, so called at Natchez, has protected the city at all. Every one must have observed, who has read Drs. Jenkins' and Duncan's articles, how much they labor in discussing certain medical points that seem to have no connection whatever with the subject. Such for instance as whether black vomit does not happen in other diseases; as if the affirmative was a common thing, Dr. Duncan introduces a negro of his, who died with black vomit at Homochitto, yet had no yellow fever. This negro was evidently introduced as an offset to the black vomit cases that were ever and anon occurring in Natchez, notwithstanding the quarantine. When the regular physicians declared that they had cases of black vomit, they were told that black vomit would occur in other diseases from the treatment or the want of it. As if such able physicians as McPheeters, Chappelier, Lloyd, Jones, Thistle, Lisle, Stone, Young, and others, did not know what the yellow fever was, and they all had cases of it as well as myself, in the face of the quarantine. If New Orleans goes for a quarantine it should have a better one than that of Natchez. The occurrence of the yellow fever in Vicksburg, soon after the quarantine was established, led to the delusion that the quarantine kept it out of Natchez. But the truth was that the epidemic of 1837 and 1839 had acclimated nearly all the inhabitants. No foreign population were coming in. No railroad improvements were going on in Natchez as in Vicksburg. The former was declining and the latter rising. Every physician knows that those, who have had the disease once, seldom or ever have it again. Hence Natchez escaped by virtue of her acclimated inhabitants during some of the years that large bodies of new comers were undergoing their acclimation in Vicksburg. By the year 1844 a few strangers came in, and the yellow fever dodged the quarantine and picked them off. So also in 1847 and 1848 there were not a great many citizens liable to the disease; yet the mortality among the unacclimated was very great, because the disease took them by surprise. By the year 1853, a goodly number had come in, and a large portion of them were swept off, notwithstanding the quarantine. The law of yellow fever, that those who have had the disease once, seldom or ever have it again, has brought a mere nominal quarantine into favor.

If we are to have a quarantine, let us have a rational and effectual one.

Science should have respect to the fears of the community, because fear itself is a potent cause of disease. While its duty is always to act on the safe side, it should set its face like flint against those costly and ruinous experiments, which have not only been repeatedly tried and failed, but served, as they did in Spain, to introduce and spread the very contagion they were intended to keep out. If the contagiousness of yellow fever be a safer doctrine to assume than its non-contagiousness, the isolation of the infected vessels and crews at a separate wharf, by permitting no others than well acclimated persons to approach them or act as stevedores, would be the most effectual and rational measure that suggests itself; the only one that has been attended with success in preventing the spread of small-pox and other contagious diseases. The Boston quarantine system would do very well—anything but the Mediterranean or Spanish quarantine. This is the Medical Society of the State of Louisiana assembled in the hall of the State University. This house was built and this Society organized, that if pestilence, mildew, blasting, whatever plague and whatsoever sickness there be, that all eyes may be opened towards this house, to learn the good way that a kind Providence, operating through the means of that Science which claims a divine origin, has left open to stay pestilence of whatever kind, and to exclude it from the land. Louisiana is a healthy State, and the city of New Orleans occupies a pre-eminently healthy location. There would be no difficulty at all in keeping pestilence out, if the evils of misrule, intemperance and oppressive taxation, which prepare the food it feeds on, and invite it into the city, were abated by a wise and cheap city government. I conclude this Report, gentlemen, by reminding you that —

“ From fact and reason we our practice draw,  
The firmest basis and the soundest law ;  
Whence nature's powers in fullest vigor rise,  
And dread disease with all his phalanx flies.”

## Acute Phthisis in the Adult.

By DR. THEODORE E. LEUDET, of Paris.

[Continued from last No.]

In the beginning of this affection there is generally a certain degree of loss of muscular energy, which increases rapidly, from the moment that febrile symptoms make their appearance; the patient becoming weak and weaker until death relieves him from his sufferings. Should the disease assume a typhoid character, this debility is much more marked than in cases where it takes on the catarrhal form. Emaciation, which so soon attracts the physicians attention in chronic or softening tubercular disease, assists us but little in our diagnosis of acute or granular phthisis. By acute phthisis we wish to be understood as meaning those cases in which the specific matter seldom or never softens; and yet causes death, not by "wearing the patient out," but by rendering a large extent of lung tissue unfit for its proper purposes by the great quantity of the deposit. In some cases, those for instance which have been called "*galloping*," death may be brought about rapidly, but the post mortem appearances of the organs are the same as would be seen had the patient lingered on for months, i. e. masses of softened tubercles, cavities, indurated tissue, etc. Such do not come under our heading of acute phthisis.

The coloration of the skin and the expression of countenance should be carefully watched, as well in the earlier as in the other stages of the disease. In the typhoid variety the eyes will be found bright though somewhat sunken, and the skin injected. This injection of the skin has not the well limited edges, etc., so characteristic of the hectic spot, and therefore may be easily distinguished from it. In some patients, where all the lesions above described are found fully developed (case 1st) the skin is often seen of an uniform rosy color, perfectly analogous to what is met with in typhoid fever, except that it is entirely free from anything like the eruptions of that affection. Pressure by the finger causes its disappearance, but it will reappear as soon as such pressure is taken off. We have never in these cases seen either lenticular or petechial spots, nor have Messrs. Andral, Gull or Johnston been more successful; but M. Waller states that in one patient he not only observed lenticular and petechial

spots, but also well developed sudaminae; the post mortem examination showing no trace whatever of intestinal lesion; but the lungs were filled with the characteristic deposit of acute phthisis. How very difficult, must it not be, to make a proper diagnosis in such a case?

The temperature of the skin has not as far as we know been thermometrically studied. Judging merely by the application of the hand, we were convinced that it was in direct proportion with the general febrile excitement; that it increased as the last scene drew nigh, although in no case was it ever so great as to give cause for complaint.

The face may retain its natural expression for a considerable time, but as the disease becomes more developed, the patient seems anxious, his skin is either much congested or pale with blue lines, etc., he lies flat on his back, or favors one side more than the other, and when the difficulty of respiration is very great, it may force him to assume a sitting position in bed, his head bent forward, exactly as we see in cases of capillary bronchitis or organic affections of the heart. In general the skin is very dry, free from moisture of any kind; in one only of our patients did we find the skin moist for several days at a time, but, never have seen anything at all resembling the periodical night sweats of ordinary consumption.

Cough, though one of the first symptoms to make its appearance in the majority of cases, and often preceding the febrile state, may only attract our attention, after the patient has been complaining for several days.

M. Louis states that in one of his patients he noticed no cough until the tenth day after the fever had set in. At first this cough is but little troublesome, the paroxysms being few in the twenty-four hours, and unattended by any severe pain; but as the last stage of the disease approaches, the paroxysms are more frequent, and the pain intense, so great, in fact, that it almost merits the name of a *convulsion* of coughing. There is often but little matter expectorated, but when any is thrown off, it always has the character of phthisical sputa. In none of our patients could we see anything like hemorrhage from the lungs; but M. Louis reports one case where a considerable quantity of blood was coughed up. Why should hemoptysis be so seldom met with in acute form of phthisis? Is it on account of the specific matter being deposited in the form of granulations? This can scarcely be the explanation, for we saw the post mortem examination of the body of a man whose death had been preceded by six hemorrhages, at each of which a considerable quantity of blood had been lost, and yet the tubercular matter found in the lungs was in the first or



miliary state, some few being slightly yellow. Nor can we explain it as dependent upon a congested state of the parenchyma of the lung, for in many such cases no such congestion exists. We agree with M. Louis when he says that "*hemoptysis* is owing to an unknown condition of things, essentially accompanied by a tubercular deposit."

One of the earliest symptoms and one which will immediately attract the attention when examining a case of acute phthisis, is the rapidity of the respiratory movements. It accompanies the first developement of the disease, increasing as the fever increases, becoming more and more distressing until the patient dies. In all our cases but one, the respirations were fifty to the minute. This dyspnoea is accompanied by a feeling of constriction but not of pain, situated at a point just behind the sternum; it is more marked in cases where the catarrhal form is present, although it is frequently seen in the typhoid variety. Pain in the side is often complained of by those suffering under this disease; in character it much resembles the stitch in the side of pleuritic inflammations. When this pain is very severe and accompanied by great difficulty of breathing, M. Andral is accustomed to call the case one of *phthisis asphyxique*.

When acute phthisis does not follow upon the chronic form, auscultation and percussion lends us but little assistance to our diagnosis, the signs and symptoms arrived at in this manner are of little or no value when considered separately. Fortunately for us, in a *scientific point of view*, the acute is most frequently engrafted upon the chronic disease, and we then have all the symptoms and physical phenomena accompanying the softening of tuberculous matter to aid us in making up our minds in regard to the state of the patient's lungs. These signs observed and described by Laennec, have been too thoroughly investigated by modern authors and are too generally known to require more than a mere mention here. Although the earlier stages of the affection we are now studying are not accompanied by any constant set of symptoms, yet in the large majority of cases there is some roughness of the respiratory murmur, with slightly prolonged expiration; but both are so little evident that no great importance can be attached to them; for we all know how easily the ear may misinterpret them. In all of our cases auscultation proved the existence of abnormal sounds in the chest; these were sonorous and sibilant rouchi, not disappearing when the patient coughed, and accompanied by a peculiar clicking noise. These râles heard, both in front and behind, are found in cases where there is not the slightest evidence of serous infiltration of the pulmonary tissue; and at a later period some subcrepitant and even crepitant

ronchi when there is no pneumonia. In patients where the dyspnœa is very great, these sounds will be heard extending over the whole of both lungs. According to Dr. Walshe, (*physical diagnosis of diseases of the lungs and larynx*, p. 139, London) when the tubercular matter exists in the granulation form we find the respiratory murmur weak in some parts of the chest and rough in others, and sometimes even loud and slightly bronchial; together with sonorous, sibilant, and sub-crepitant râles. As usual in all cases wherever the ronchi are loudest there will we find the voice more resonant. Drs. Louis, J. Clark and Walshe support this opinion. The stethoscopic signs of acute phthisis are therefore principally remarkable for the extent over which they are heard as well as for being more developed in some parts than in others, although we have met with the disease where they were heard over a very limited space. Of course when softening of the specific matter begins new signs are heard, and our diagnosis becomes much less doubtful.

As a general rule the digestive tube remains free from lesion. We have never seen in the intestines any pathological condition which could be considered a consequence of, or in any way connected with, the lung disease. But notwithstanding this, our patients often call attention to the alimentary canal by their complaints of thirst, want of appetite, disgust for all kinds of food, bad taste in the mouth, pain in the pit of the stomach, etc. One of our cases complained of nothing but pain in the stomach and foul mouth for a considerable time before other symptoms were observed.

The tongue is sometimes uniformly covered by a yellowish white coat; at others this only exists in the central portions, leaving the edges and tip free and slightly tinged with red. In cases of a typhoid character the whole mucous membrane of the mouth assumes an appearance somewhat analogous to that which is met with in the fever. Vomiting is of rare occurrence, we have never met with it in the beginning of the attack, and twice only during the progress of the disease.

Diarrhœa, contrary to what is observed in ordinary consumption, is seldom seen in cases of the acute disease; some one or two of our patients had two or three loose stools per day; the others were perfectly regular in their bowels. There is generally some tympanitis during a few days before death, but never have we met with liquid in the peritoneal cavity. Of all writers on this Dr. Waller is the only one who has ever found the spleen enlarged in acute tuberculosis.

In a former portion of this paper we mentioned that the disease was ordinarily accompanied from its very debut by febrile symptoms. It is in fact one of the earliest signs of its developement. This fever is rarely preceded by chill or followed by sweats; the patients generally describing it as a simple increase in the temperature of the skin. We have always seen it as thus described, where the disease made its appearance while the patient was in the hospital under our immediate observation. In looking over our notes we find that in the large majority of the cases the pulse was over 80 beats to the minute, rapidly rising to 100, 110, 120 and even 140. From this we see that the pulse was as quick as in the most inflammatory affections. One interesting fact which attracted our attention is, that the number of pulsations is in direct proportion to the number of respiratory efforts. The temperature of the skin itself is actually increased; we therefore may assert without fear of contradiction that in acute phthisis we have the whole assemblage of symptoms to show a well marked state of febrile excitement.

As the brain often becomes affected even from the very beginning of the attack, we should be perfectly familiar with the fact, in order that we should not be led astray in our diagnosis by the resemblance it bears to true inflammation within the skull or to those low forms of fever accompanied by head symptoms. Of course, we do not now speak of the cases in which tuberculous matter is deposited upon the meninges; such complications we will study separately. The cerebral symptoms we now speak of are, general trouble in the intellectual faculties, loss of hearing, of sight or false vision and delirium in its different forms. These are generally observed in cases of a typhoid kind. One of the first symptoms is a general indifference to all that surrounds the patient, (observ. 1st, Louis obs. 39) then slight delirium without fixed ideas, with general loss of intelligence. According to M. Fournet, delirium in phthisis is a sudden and intense exaltation of all the faculties, without any special direction being given to them. One of our patients died in a comatose state following this sort of delirium. In speaking of the different modes of termination of the disease this last symptom will be brought up again.

[*To be Continued.*]

## Glycerine in Tuberculosis.

*By J. L. CRAWCOUR, M. D., New Orleans.*

Honorary Corresponding Member of the Society of Arts of England,

In a communication which appeared in the January number of this Journal, I stated that I had found glycerine not only a safe, but a most useful remedy in many cases of disease. I now purpose to examine more in detail the grounds for its adoption in tuberculosis. Hitherto, the sheet anchor of the profession has been cod liver oil, and although this in many instances has proved an invaluable agent, still in many other cases it has disappointed our expectations, and on many we have been deterred from its use by the invincible repugnance of the patient, by the nausea it frequently induces, and by the extreme difficulty with which it is sometimes assimilated. In no disease is dyspepsia so formidable a complication as in phthisis, and in no other disease do we find so much difficulty in restoring the digestive organs to a healthy condition. It becomes, therefore, a matter of great importance to the practitioner, to find some agent which, while it possesses the remedial virtues undoubtedly existing in cod liver oil, should at the same time be free from the objections militating against its use. Modern chemistry, among many of the boons it has presented to medicine, has shown us that in a few grains of the alkaloid quinia, may reside the powers of an ounce of bark, that in a minute atom of atropine may be present all the useful properties of a comparative large dose of belladonna, and it is probable that a similar course of investigation may demonstrate to us, in what really consists the superiority of cod liver oil as a medicament to other oils. That it is not solely as an oil, that it exerts its influence, is evidenced by the fact that other oils, as oils, will not produce the same effect, and it is not to its combination with iodine, a bile constituent, that its power is due, is proved by the fact of these agents failing to produce the specific therapeutic action of cod liver oil, even when combined with fatty substances. But there is one constituent, which, though common to all oils, yet exists in such large quantities in cod liver oil, as at once to direct our attention to it. I allude to glycerine, the base of all animal fats, and which is not only present in a greater excess in cod liver oil, than in most other oils, but is found in a larger proportion in that variety of oil, which modern experience has pronounced to be the most easily assimilated, and the most efficacious — I mean the pale oil.



The following are the proportions of glycerine, in 100 parts of cod liver oil :

100 parts of brown cod liver oil, contained	9.111 of glycerine.
" light brown, - - - - -	9 073 "
" pale do - - - - -	10.117 "

this later variety showing a marked increase in the quantity of this constituent.

It may be stated as a rule, that the more nearly a substance resembles the organism into which it is to be introduced, the more easily will it be assimilated, and the more easily will it supply the wants of that organism. Now, glycerine forms the basis of all the fats of the human body, and lactic acid is found in all the juices of the body. If we examine the analyses of these two substances, we shall be struck by a curious chemical coincidence. Lactic acid is composed of C6, H5, O5. Glycerine of C6, O5, H7; or, may be hypothetically stated to be lactic acid, by 2 equivalents of hydrogen. Grape sugar, sugar of milk and starch, severally consists of C12, H12, O12, or 2 equivalents of lactic acid, or of 2 equivalents of glycerine, minus 2 equivalents of hydrogen, and are all converted into lactic acid, before they can be used for the economy. Now, lactic acid is decomposed by the respiratory process, and according to Dr. Carpenter, may supply a pabulum for the combusive process, and thus contribute to the maintainance of the heat of the body. If lactic acid supplies this indication, glycerine which contains two atoms of hydrogen to spare, and which it readily yields up, must act more efficaciously; and as one of the sources of lactic acid to the system, (besides the transformation of starchy and saccharine food,) is due to the destruction and metamorphoses of muscular tissue; we may account for the wasting in phthisis, and for the remarkable benefit seen so early after the use of cod liver oil, without supposing that the power is due solely to the absorption of the fatty materials of the body, or that the latter is due solely to the assimilation of the carbonaceous material of the oil.

In pulmonary disease, one object of treatment is to exhibit an agent on which the oxygen of the air may act, instead of upon the tissues of the patient, and to fulfil this indication, we select from the unazotized, or combusive class of food, and probably, except with regard to readiness and ease of assimilation, it makes very little difference, which we choose, whether starch, sugar, fat or alcohol; but as in this disease, the assimilative powers are highly impaired, it becomes the great desideratum to exhibit that, which shall *cæteris paribus*, give the least work to the digestive

organs, be the most readily absorbed, and contain the largest amount of necessary material in the least bulk. Looking to this view of the case, Dr. Turnbull, of London, recommends the sugar of milk, and for the following reasons :

“It appeared to me,” says Dr. Turnbull, “that as the functions of the lungs are more or less impeded in all pulmonary diseases, and as cod liver oil has been found so beneficial in consumption, advantage would be gained by selecting from the non-azotozed class of alimentary substances, such of them as would have the greatest tendency to unite readily with the oxygen absorbed at the lungs, and thus in the disabled condition of these organs, to facilitate the performance of their functions. I was thus led to inquire which of the combustive class of alimentary articles are more readily digested, and have the greatest affinity for oxygen. Sugar of milk, possesses these properties in a high degree, and whey, which consists almost entirely of sugar of milk, has long been found useful in consumptive cases.”

For these reasons, Dr. Turnbull suggests the employment of sugar of milk in consumption. Now, if these views are sound, and these remedies should be found useful in disease, certainly for similar reasons, glycerine should occupy a prominent position, especially when we take into consideration, the fact of its possessing two atoms of highly combustive material, viz : hydrogyen, over and above that portion of its constitution, which is isomeric with lactic acid and sugar. That it is not to the preponderance of carbon only, that many remedies used in consumption, owe their efficacy, is proved by several experiments instituted by Dr. Theophilus Thompson, of the Hospital for Consumption, London. This distinguished physician, in his experiments on the therapeutic effects of various oils, arrived at the following results. He found that olive and almond oils, unless combined with phosphorus, produced negative results, the oil of the sun flower seed, which contains a very large amount of carbon, was equally unsatisfactory, but from cocoa nut oil, he obtained results equally favorable with those obtained by the use of cod liver oil, and the increase in the amount, of red corpuscles was equally under the use of either oil.

The oil employed was a pure cocoa oleine, obtained by pressure from the crude cocoa nut oil, refined by being treated with an alkali, and then repeatedly washed with distilled water. It burns with a faint blue flame, showing a *comparatively small proportion of carbon*, and is undrying. Now, if we compare these several oils, what do we find? Cod liver oil, a highly complex substance, and active in its therapeutic effects. Sun flower

oil, containing a large excess of carbon inactive, olive and almond oils simple in their constitution, and containing a moderate quantity of carbon, comparatively inert, while cocoa oleine equally simple in constitution, and equally moderate in carbonaceous elements, rivals in therapeutic activity, the complex cod liver oil. The following are the proportions of carbon, these oils contain : cod liver oil 80.18, equivalents; cocoa aleine 69.62; oleine and almond oils 69.38. The amount of benefit derived, is out of all proportion to the carbon contained, for if it were not so, butter and lard would be equally beneficial with cod liver and cocoa oil, but though to a certain extent, all oleagenous substances are useful, still some seem to act more efficacious than others, and possibly this may depend upon the ease with which they are assimilated, and the rapidity with which they may be taken up by the system. Now, glycerine is so simple in its constitution, so miscible with all fluids, so homogenous with the system itself, that it is highly probable that like other fluids, it may at once be taken up, and conveyed in the torrent of the blood throughout the whole system, and supply the want of oleagenous material required, and that without fatiguing the already debilitated digestive organs. Its low specific gravity 1.28, not much above that of blood 1.056, favors this assumption.

On examining more closely into the cocoa oleine mentioned by Dr. Thompson, I think we shall come to the conclusion that its efficacy above other oils, does really depend upon the large amount of glycerine it contains, for although we have no qualitative analysis of it, yet its physical properties so closely resemble those of glycerine as to warrant us in this assumption. In the first place its mode of preparation, viz : the purification by means of an alkali, in other words its incomplete saponification, would tend to remove a large amount of the fatty acids and leave the glycerine comparatively free; and secondly its mode of combustion is not that of an oil, but exactly corresponds with what we observe when glycerine is ignited, for this also burns with a pale blue flame. I am disposed to believe therefore that, as in the Peruvian bark, its chief efficacy depends upon the quinia it contains, and as in the old fashioned remedy the *spongia usta*, its remedial virtues were due to the iodine its constituent, so in cod liver oil and cocoa oleine the active principle may be said to reside chiefly in the glycerine they contain. If this be so, the advantages that glycerine possesses over both these remedies, to agreeableness of taste, in case of assimilation and above all in the property it possesses of entering into combination with almost every article in the *materia medica*, excepting ether, must warrant its adoption in their place, where, as in phthisis, a remedy has to be continued for months and perhaps for years, it

becomes almost an absolute necessity that it should be as little repugnant to the patient as possible, and if of two remedies, both possessing equal claims to attention, one has the superiority in the article of taste, it becomes our duty to employ it. With regard to the exact mode in which these substances act, little is known with certainty; they certainly do not act exclusively in the way of nutriment, but in the words of Dr. Thompson, "they seem to modify the condition of the granules which enrich the blood and to dispose them to the calm progression of change by which they are made to contribute to the production of healthy structure."

I mentioned in my former paper, I had found that phosphorus was to a certain extent soluble in boiling glycerine, and that the solution was nearly tasteless, and miscible with water or other liquids. This I think, is a circumstance well worthy of attention, as although phosphorus has often been required as a remedy, the difficulty of administering it, has hitherto been so great, as almost to banish it from the materia medica. In phthisis, it has been strongly recommended, so much so indeed, that many are still disposed to consider that the remedial power of cod liver oil, consists in the amount of this substance it contains, and it has certainly been found efficacious in the disease; when we consider that the blood in health, as demonstrated by Dr. G. O. Rees, contains a phosphorized fat, which, according to his theory, when exposed to air in the lungs, absorbs oxygen, and is converted in phosphoric acid, and this becomes a source of heat, one shall be at no loss to conceive why it should prove in many instances so valuable a remedy, especially when we recollect that this excess of phosphorus is chiefly found in the red corpuscles, which become remarkably diminished during the progress of phthisis; this gives us further evidence of the necessity of supplying this element, and I therefore suggest the employment of the glycerole of phosphorus, in combination with glycerine as an efficient and agreeable substitute for cod liver oil in phthisis.



## Observations on Ligature of the Gluteal Artery.

*By WARREN STONE, M. D., Professor of Surgery, University of Louisiana,*

WITH A RECENT CASE,

*Reported by A. S. FOX, Resident Student, Charity Hospital.*

John Spark, a boy of ten years, from the town of Galveston, Texas, was brought to the Charity Hospital on the evening of the 6th of Feb-



ruary, having a large pulsating tumor in the gluteal region. Dr. Randall, who accompanied him, stated that on the 28th of December previous, a penetrating wound had been inflicted in the gluteal region by a narrow bladed knife, some hemorrhage ensued which had ceased of its own accord when he saw him two hours after. The wound healed kindly and the patient was out again in three days, and rode on horse back, had no soreness in the part. On the evening of the 3d of January he was seized with a lancinating pain in the gluteal region, and pains in his knee and ankle of the same side on the 4th. Dr. R. saw him, discovered no signs of aneurism. 5th, Dr. R. saw him again and discovered a tumor in the gluteal region pulsating isochronous with the arterial diastole. This he discovered to be a false aneurism. In spite of compression and every means he used to prevent its increase, it grew rapidly for five days and then ceased to increase so fast. Dr. R. now proposed to operate by tying the gluteal artery, but the scientific of Galveston did not seem disposed to acquiesce and assist in the operation, consequently he desisted. The pain grew more severe and the patient began rapidly to decline from loss of sleep and suffering, so the Doctor resolved to bring him to this city. Large opiates were administered pro re nata to procure rest, and the patient arrived on the 6th of February—presenting the following symptoms. Pale and emaciated; pulse 140; very feeble; loss of appetite; a large tumor in the gluteal region pulsating isochronous with the arterial diastole; severe pain in the hip, and a numbness throughout the leg with occasionally starting pains in the knee and ankle. A large dose of morphine was given him at night to procure rest. Dr. Stone saw him on the following morning and determined to ligature the gluteal artery. Accordingly having had the patient placed upon the table he proceeded—in the presence of the medical class—assisted by Drs. Choppin, Beard and Randall. He made an incision across the top of the tumor following the course of the fibres of the gluteus major muscle, eight inches in length, scooped out the coagulated blood, the sack being freely opened, and placed his finger upon the artery to control hemorrhage—a ligature was then placed around the artery, and a compress of lint gently applied to stop a slight oozing which existed from the neighboring parts. The wound was drawn together by adhesive straps, an opiate was given and the patient put to bed.

8th. — Patient is doing well—pulse increased in volume, not so fast and complain of no pain, except of lying on one side.

9th. — Looks much better—countenance looks brighter—appetite improving—pulse 120.

10th. — Still improving — has had each night since the operation, a small dose of morphine to procure sleep; think it is required more from habit than from pain.

11th. — Much better; appetite good; had an operation on his bowels this evening; slept well during the night; wound is supurating freely.

12th. — Still improving, has no pain except from lying on one side; appetite still good; dressed the wound with a weak solution of chloride of soda; required very little morphine last night.

13th. — Still improving.

14th. — Looks very well, but has a tendency to diarrhœa; this yielded readily to a little chalk mixture with catechu; continued doing well till the 19th; his pulse being small and feeble, with some tendency to febrile paroxysms towards evening. Dr. Stone ordered him a little quinine, after which he improved rapidly. The wound being nearly healed, and wishing to return home, he was discharged on the evening of the 24th February.

A. S. F.

The above case is interesting, as there have been but few cases of wound of the gluteal artery recorded. Malgaigne states in his *Operative Surgery*, that it has been tied only four times. A case is related by one of the Bell's, and one by Carmichael, of Dublin, (I think,) in both of which, the artery was tied by laying the parts freely open, as in the above case.

This is the third case that has come under my care. The first case was brought into the Charity Hospital in 1835, considerably exhausted by the loss of blood, from a wound of this artery; it was treated by compression. The wound healed, but about two weeks after, a small tumor was discovered in the region of the gluteal artery, which, upon auscultation, gave the usual aneurismal murmur. The tumor seemed dense, as if considerable lymph had been thrown out and organized for its protection, and pulsation was only observed when its walls were compressed. This patient remained in the Hospital about six weeks, and as the tumor did not increase, he was discharged; being warned of his condition, and advised to return if the swelling should increase; but I heard no more of him. The second case, was admitted into the Hospital in 1853, completely exhausted from the loss of blood. The wound was made six days previous to my visit to him, and he had suffered repeated bleedings. At the time I first saw him, he appeared perfectly bloodless, and nearly pulseless, having just had a hemorrhage, and it was thought that he could not bear the operation. Stimulants were administered, and in about an hour, the pulse became steady; and as the operation offered the only possible chance for life, he was placed

upon the operating table, and the artery tied. The operation was made by making an incision from near the posterior superior spine of the ischium, to the upper part of the trochanter major, (which, from the distention of the parts by the coagulum beneath the muscle, was near a foot in length,) the coagulum was scooped out, and the finger placed upon the artery just where it emerges from the pelvis. In the upper part of the ischiatic notch, it was found that the artery was cut very near to the finger, and it was with great difficulty that a ligature was placed upon it; this was finally done, and the patient was carefully stimulated and nourished; but in spite of the greatest care, gangrene took place in the wound in consequence of the great loss of blood, and he died on the fourth day after the operation.

The surgery of the arteries, is generally considered complete, and it may be true in general terms; but it is certain, that wounds of arteries are frequently occurring, for which, no precedent is recorded, and in which differences of opinion will exist, as in the case detailed by Mr. Fox. By most surgeons, it is considered sufficient to tie the artery on the proximal side of the wound; but Mr. Guthrie, insists that it is important to tie the artery on both sides of the wound, at least when it occurs in the arteries of the extremities, as the free anastomosis he asserts, renders hemorrhage very certain from the distal extremity. I have tied most of the large arteries for wounds, and in most cases have applied a simple ligature at a convenient place on the proximal side of the wound, and have never been troubled with hemorrhage from the distal orifice, except in one case. This was in a wound of the planter arch, in a case that was admitted into the Hospital, sixteen days after it was received. The patient, a little boy of ten years, was completely exhausted from repeated hemorrhages, and I tied the two tibial arteries, the anterior one on the dorsum of the foot, and the posterior one behind the inner maleolus. Three days after, he bled again, but not very freely, and it was thought a little pressure would command it; but the next day, it recurred with more violence, and the patient was completely exhausted, and evidently could not survive any further loss of blood. The foot was swollen and painful, and it seemed impossible to secure the artery in the sole of the foot, without destroying the organ, and I determined upon tying the femoral artery, which was done without the use of chloroform as the patient lay in his bed, for he was so weak, that I feared to disturb him; this secured the bleeding, and the little patient recovered perfectly.

But notwithstanding the confidence which my experience gives me in the safety of tying the artery at a convenient place on the proximal side

of the wound, I would advise, in cases of false aneurism, when there is considerable extravasation of blood, to lay the sack open freely, and tie the vessel at the wound, whenever it can be done without cutting through important parts. The impression is, that a large sack or cavity containing blood, laid open and exposed to the air, will take on ill-conditioned inflammation. This is generally true, where the extravasation is from a contusion, but there is great dissimilarity in the cases. In the former case, the blood is isolated, and can be scooped out clean, and the cavity heals kindly; but in the latter case, the blood mingles and clings to the contused parts, and cannot be sundered, and when exposed to the air decomposes and becomes a poison. Another fear that deters from opening the tumor is, that the artery may not be readily found, and the patient suffer from loss of blood. When the artery can be commanded by pressure on the proximal side, there can be no objection, and the bleeding mouth of the wounded vessel is readily discovered. The flesh is pushed away by the pressure of the extravasated blood and leaves the mouth of the vessel isolated, compared with its position in a fresh open wound. The advantage of this method of operating is, that we get rid of the mass of blood, which will generally have to be discharged sooner or later by a separate opening, when the quantity is large, and we make the operation more certain, for the sack may be fed when the anastomosis is free.

In the case related by Mr. Fox, it was suggested to me that it would be best to tie the internal iliac, as the little patient was too much exhausted to bear any more loss of blood, and the uncertainty of finding the artery at once would render the operation dangerous. But from my experience, I felt confident of being able to command the bleeding, though I expected great difficulty in tying it, from its great depth. It would have been much easier to have tied the internal iliac, but the hip must have been laid open, for the pressure of the immense clot was causing great suffering, and the patient could not have rallied from the two wounds, to say nothing of the danger of secondary hemorrhage. False aneurisms are not always readily detected. The bleeding orifice will occasionally be blocked up with a clot, and no thrill or pulsation can be discovered, and this may occur repeatedly.

A stout, athletic young man called on me at the Charity Hospital, in 1837, for a lame shoulder. He stated that he fell, two days previous, from a high bank on the river, and forcibly dislocated his shoulder, which some men on the boat on which he was employed succeeded in reducing immediately after. The arm continued very lame and painful, and he called on



me to see if it was in place. I found it in place, but much swollen. He returned the next day, when I found the swelling much increased, and a sense of semi-fluctuation was observed in the axilla, such as is peculiar to coagulated blood. I carefully examined and auscultated, but could make out nothing. He was admitted an in-door patient, and carefully watched, for I was sure that a blood vessel had been ruptured. I examined him several times a day, but it seemed singular that the tumor increased at night and remained stationary during the day, and sometimes it seemed to diminish. The pulse at the wrist was nearly or quite as good as in the other arm, but the tumor continued to increase until the skin appeared ready to give way, when I punctured it, (so that I might be present when it gave way,) and forced out some coagulated blood, but no fresh hemorrhage took place, and I placed a trusty man by his side, with proper instructions if it should bleed. I returned in an hour or so, just as the clot gave way, and a jet of blood took place. It was red arterial blood, and flowed with a jet. There was now no mystery; the axillary was wounded, and I resolved on tying the subclavian above the clavicle. This was done with great difficulty, for the shoulder and clavicle was forced up by the great mass of coagulated blood in the axilla. I succeeded, however, in getting down to the artery, but could not see it; but having an aneurismal needle, with a flexible silver point, into which the needle screwed, I shaped the point into something like a cork-screw, which I was able to carry under the artery, and seizing the point with a pair of forceps, the handle was unscrewed and the point was drawn through, and the artery tied. The opening in the axilla was enlarged and a portion of the coagulated blood discharged so as to relieve the pressure, and the patient did well for fifty or sixty hours when he was seized with pneumonia (which was prevalent at the time) and died in the beginning of the fifth day. Dissection showed that the subscapular artery had been torn off smooth with the axillary trunk, and there were a number of clots in the sack which had evidently formed over the orifice from time to time as the force of the circulation became weakened or the sack distended, so as to resist the flow of blood, and cast off when the parts had become relaxed by yielding to the pressure; and hence it was that the real nature of the injury escaped detection, although suspected, and frequently looked for. It is worthy of remark that the arm of the affected side maintained its temperature nearly as high as the other, and the parts were evidently sufficiently nourished although all direct circulation was cut off, and by the loss of the sub-

scapular artery the circulation from the supra-scapular was cut off. This shows that parts may be nourished by permeation or at least, blood may get to parts by unknown channels. It is very certain that if the nature of this injury had been discovered early, and the artery tied before any considerable amount of blood had been extravasated, and the patient exhausted, he would have had a fair chance for recovery.

Another case (which was referred to in a previous article, to illustrate the method of compression,) was brought to my infirmary. A large semi-fluctuating tumor was found upon the left side of his neck, occupying nearly the whole space from the mastoid process, to the clavicle; it evidently contained coagulated blood. His history was, that about five months previous, he was stabbed with a narrow knife in the neck, and lost a large amount of blood. The hemorrhage was arrested, and the wound healed; but a tumor formed, which had gradually increased, until he came under my care, when it was very painful and appeared as if the integuments would soon give way. I had no doubt but it was a bloody tumor, but a careful examination showed that the carotid artery, was not involved; there was no pulsation, and auscultation showed no sign. Careful examinations were made several times, and as the wound of the vertebral artery did not occur to me, I conclude that the tumor was formed, and fed from one of the cervical arteries, and determined to lay the tumor open, scoop out the blood, and tie or compress the vessel. An incision was made, and as soon as the pressure was relieved by a discharge of some of the coagulum, the blood gushed in torrents. The wound was rapidly enlarged, the blood scooped out, and the finger thrust to the bottom of the sack, and between two of the transverse processes of the vertebræ, and the bleeding controlled. The end of a long strip of lint was passed down with a blunt probe, to the bleeding point, and carefully pressed down, and folded backwards and forwards until a cone was formed, filling the wound with its base at the surface of the wound, and the apex resting on the mouth of the wounded artery. This completely controlled the bleeding, and the compress was left with very little pressure to be pushed out by the granulations. This case recovered perfectly. I do not think, with the experience I then possessed, that if I had discovered a strong pulsation, and a loud murmur, that I would have ventured to lay it open as I did, and the patient, no doubt, would have died of hemorrhage when the sack gave way. We may be deceived in the same manner in true aneurisms, the pulsation and murmur may be arrested from time to time by the formation of clots. So we should be careful in forming a diagnosis where there is any suspicion of aneurism.

But to return to the treatment of false aneurism, I have tied the brachial artery twice for aneurism at the bend of the arm produced by puncturing the artery in bleeding, and although the tumor was not deep in either case, it was slow in absorbing; in fact never entirely disappeared, (at least so long as under my observation) and the motion of the arm was much impaired. I am satisfied that the cure would be much more certain and perfect if the sack were laid open, the artery tied, and the clot discharged. I have tied the brachial artery for wounds of the ulnar artery under the bellies of the flexor muscles of the fore-arm and have been obliged generally to enlarge the wound to discharge the extravasated blood and the same in wounds of the femoral artery. The truth is, physicians are not usually prompt enough in the treatment of wounded arteries. If an artery requires tying there should be no delay. When there is no immediate danger from hemorrhage we may be justified in trying compression, but if it fails and bleeding occurs after a few days, whether it flows externally or is confined in the tissues and forms an aneurism, the artery should be tied at once, for the case is sure to go on from bad to worse, and the patient's chance of recovery daily diminished. If the sack can be laid open or the wounded artery exposed and tied at the wound without dividing important parts, it should always be done, but if not it should be tied at a convenient place on the proximal side of the wound, and subsequently the extravasated blood can be discharged, if necessary, either by enlarging the original wound or by a new opening, as may suit the case. I feel confident that I lost an arm many years ago by not discharging the blood extravasated under the muscles of the fore-arm. The ulnar artery was cut by a narrow chisel, thrust through the bellies of the flexor muscles of the fore-arm; it had been treated for a week by compression, and the patient had lost considerable blood by repeated bleeding, and there was a large amount of blood extravasated into the intermuscular spaces; I tied the brachial artery, gangrene occurred and I had to amputate. On examination of the limb I felt satisfied that the gangrene was from the compression of the extravasated blood interrupting the collateral circulation.

## Robin's Lecture on the Physical Characters of Pus.

*Reported by G. VANCE, M. D.*

A proper knowledge of pus, with its microscopic elements and reactions, is of too great importance to the pathologist to require any apology for offering the present paper to the medical public; containing as it does the result of investigations carried on by one of the most distinguished micrographists of Europe.

The fluid known as pus is of a creamy consistence, greenish yellow in color, and composed of globules floating in clear serum. When the cells exist in large proportions the liquid is thicker, of a darker tinge, and is then known as laudable pus; if, on the contrary, the serum be in excess it is thin and watery and known as unhealthy pus. The elements of this liquid are:

- 1st. Globules.
- 2d. Oil drops.
- 3d. Exudation corpuscles.
- 4th. Grey molecules insoluble in acetic acid.

The globules are of two kinds—those *with* and those without nuclei. The first are generally perfect spheres, unless taken from a living person, as in case of abscess, etc., when they are found more or less irregular, but becoming spherical as soon as the liquid cools or a little water is added. Their diameters are variable, averaging about the ten to the twelve thousandth of a millimeter, some few measuring even as high as the seventeen thousandths; these latter are oftenest met with in pus taken from abscesses, inflamed lymphatic glands, ulcers of the legs, etc. It may be laid down as a general rule that when the globules are formed rapidly, as in acute inflammatory action, they are smaller than when resulting from chronic inflammation. In healthy urine a cloudy deposit is often observed, settling to the bottom of the vessel containing it, which, when examined under a microscope, is found to consist of epithelial cells in considerable numbers, together with a few pus corpuscles; these latter are of the smallest size ever found, being scarcely the nine thousandths of a millimeter in diameter.

These peculiar bodies found in pus, are generally transparent and colorless; their surfaces are perfectly smooth, and not irregular and uneven as some authors have described them; this idea, no doubt, originating from the fact of their having been first studied with low magnifying powers,



and the granulations which are now known as mere accidental adhesions of the molecular particles floating in the serum, being mistaken for essential parts of the globule.

*Reactions.*—When placed in water they swell out to double their ordinary size, and become more transparent. Acetic acid also increases their volume making them perfectly diaphanous. Many authors consider that this takes place through the property the acid has of dissolving the globules. That they are dissolved by it there cannot be a doubt, but from thirty to forty minutes are required before this happens, and yet we see the globule become transparent as soon as it is brought in contact with the acid. The nuclei are immediately made visible but are by no means set free.

*Nuclei.*—These are not always regular in shape, although often seen perfectly round; their outlines are dark and well defined; and they may generally be distinctly seen without the aid of water or acetic acid; yet at times they are completely hid by the large quantity of granular and amorphous matter which covers them. They vary in numbers, from one to five in each globule, the average being about three. In phlegmonous pus, three or more are generally found; but in that, secreted from the surface of mucous membranes, two are as many as are ever met with. In these, the nuclei are always easily seen, without the assistance of any reaction. In size, they are from two to three one thousandths of a milimetre. Those bodies which have been described by many microscopists as mucous globules, have no real existence. They are simple pus cells, found in the mucous secretion.

*Reactions.*—The nuclei are not acted upon by chemical agents, except being made more distinct by the increased transparency of the substance in which they are imbedded.

*Globules of the second variety*, or those without nuclei, were first described by Lebert, and called by him pyoid bodies; and may be found in varying numbers in nearly all kinds of pus, but principally in that resulting from inflammation of serous membranes, where they exist in large majority over the other kind. Pus furnished by the skin, and by the internal membrane of inflamed veins, also contains a large number of these pyoid bodies. They have the same general characters as those of the first class, are rather more transparent, and have no nucleus. The reactions are also the same.

*Fatty Granulations.*—There are two kinds of granulations found in pus; the one of a grey color and acted upon by acetic acid; the other, of a

brilliant yellowish tinge, and insoluble in the acid. They float freely about in the serum, but may at times be found buried in the substance of the globule, to which they then give an irregular and opaque appearance.

(M. Robin here entered upon a series of arguments, to prove that these granulations, the pus globule, the cholestrom body, and the exudation corpuscle, were not formed by transitions from one to the other.)

In studying pus, we must remember the resemblance which the globules bear to the white corpuscle of the blood. Of the latter, there are two kinds: those with nuclei, as in foetal blood, or in that state of the fluid called leucocythaemia; and those without, as they are found in the adult. The principal points of difference are: in size, the white globule being only eight thousandths of a millimetre in diameter, or one fourth to one half smaller than the pus cells; in reaction, when acetic acid is added to the blood discs, they become much more granular; these granulations, in five or six minutes' time after the application of the chemical agent, collect towards the centre of the globule, and assume a peculiar and quite characteristic reddish color; when pus is treated in the same manner, the nuclei remain colorless, and the globules perfectly transparent. The pyoid bodies are easily distinguished from all others in being essentially diaphanous, with no trace whatever of a central body.

*Pseudo Pus.*—This contains none of the elements of pus; the name has therefore been improperly applied. The fluids which have been so called, are found in nearly all cases to be serum, holding in suspension a considerable number of epithelial cells.

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## Editorial and Miscellaneous.

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Except the primary relationships of domestic life, there is none among men more near, than that of Physician and Patient. It may almost be said that the physician forms one of the domestic circle; for this abode of all man holds most dear, is his peculiar province. When invaded by disease, his aid is invoked to drive away the destroyer. The agonized husband, the helpless mother, the despairing children, wait patiently his coming; at his approach, hope revives in their breasts; and we have seen even the darkest gloom seem almost to smile at his presence. They watch his boding countenance, and their hearts mark every change; their smiles wait upon his, and his anxiety, makes theirs tenfold more intense. For the time, indeed, he seems to be the soul, upon whose motions, they

all, as obedient members, wait. When peace is restored, he is blessed as a conqueror, and, better still, as a dearest friend. And in the most adverse fate, if he be one whose skill and faithfulness have long been tested, his want of success is imputed to the irresistible course of nature, directed by the will of God.

To him, this holy circle is always open. He comes and goes, night or day, in sorrow or in gladness. The servants are at his beck, and with eagerness, and respect, obey his call. From him there are no secrets; his office makes him an inquisitor, to whom modesty itself must reveal its most sacred knowledge. Towards him there is no jealousy, no envy. The husband confides to his honor and skill the sanctities of the marital relation; the father, the lover, the brother, all place what they esteem most precious in his hands. But the relationship approaches closer still, "yea all that a man hath will he give for his life." And this precious life, every man must, at some time or other, place under the protection of the physician. Then, his family history, their constitutions and predilections, and the vices and indiscretions of his own youth or age, must be told perhaps, (and sometimes through fear, he exaggerates them,) and the man stands before his physician all exposed, as at the judgment bar, and finds himself utterly in the power of another. He discovers to another all his little meannesses and weaknesses, who has heretofore shrouded them almost even from himself. Yet it is without fear of consequences. He tells not only a superior, but a friend; and waits with trembling hope for his judgment. He attends to counsels, and obeys implicitly who never brooked direction before. No self denial is too hard, no pain too severe for him to undergo, that he may gain his precious health. Ah, poor creature! his physician is to him, perhaps, in place of God.

No wonder the Orientals and the Indians regarded a physician as a holy favorite, and friend of the Gods; and Jesus, the son of Sirach, saith — "Honor a physician with the honor due unto him, for the uses which ye may have of him; for the Lord hath created him. For of the Most High cometh healing, and he shall receive honor of the king. Then give place to the physician — let him not go from thee, for thou hast need of him. He that sinneth before his maker let him fall into the hand of the physician."

Yes, his office is the prime necessity of deranged nature; and the very capstone, next to that of the great physician of souls, in the new order consequent upon the fall of man. It rules over all, high and low, rich and poor. From the sobs of the loneliest orphan babe that ever wailed its life out upon the midnight air, to the feeble gasps of old age, and the incoherent mutterings of the mind diseased — all fallen suffering nature calls to him for help. And yet he is a man; alas, he is but a man! He has not been gifted with intellect superior to all others; has no familiar spirit to prompt him; has entered into no league with Hermes or Esculapius. He necessarily has doubts and waverings, half born ideas, nervous trepidations, and bitter prejudices, more perhaps, than any other professional man; and the attractions of pleasure, and the enticements of idleness have often, it may be, to his bitter regret, tempted him to neglect his necessary studies. His heart beats sad pulsations with every cry of distress; and he sympathizes with every pang of anxious love. In fine, he is a man of like capacity, frailties, and tenderness with



his fellows. Ah, what does not the relationship involve with him! His honor, conscience, pride, and all the noblest attributes of his intellect and affections, are enlisted in his profession. Alive with all these, he is called to the bedside of the dying. Parents, children, friends, the patient, and all the affections of his own soul within him, call tumultuously to him for help. Overwhelmed by the fearful responsibility of his position, he calls hurriedly on nature, science, invention, memory, for help—but there is no help. Expiring nature stretches out no hand; science has expended its knowledge; invention has exhausted her expedients; memory tries a vain task; and he steps aside for death to do his work: and unable to scan the mysterious decrees of Providence, mistrusting his own powers, and doubting the truth of his own science, he is ready to exclaim: God alone can perfectly fill my office!

There are improvident and brutal husbands, and unfaithful wives, unnatural parents, and ungrateful children, cruel masters, and bad servants; that with physicians and patients, there should be mutual infractions and neglects of duty, is to be expected. This relationship has, like the others, been debased by many to an affair of money. The one living only to receive and spend it—the others, thinking their whole duty performed, when they grumblingly pay it. But the true physician undergoes labors and sufferings, for which money cannot pay; and the right minded patient receives benefits, which he feels that money cannot compensate.

Much has been written about the duties of men in other relationships, and in some they have been prescribed by the State; but, little has been said about medical ethics, which seem to have been left to the simple dictates of nature, while the State has abandoned its citizens to be victimized by every idle, indolent, ignorant fellow who chooses to place “Doctor” before his name. As for quacks, and careless physicians, (who are little better,) when we attentively consider their position, we cannot help but look upon them as the most striking instances of the depravity of man. Only their lack of intellect and the bluntness of their consciences prevents them from regarding themselves as the most cold-blooded of murderers. Worse than highwaymen, they take the money and the life too.

The code of medical morals involves in it, in their greatest force, all the requirements of the decalogue, relating to the duties of man to man. Its great first rule is, that the physician shall devote to his profession all his heart, soul, mind and strength, and shall do nothing to dishonor it. But when we look at the medical fraternity in this country, in how many instances do we find this command disobeyed. Pleasure, business, idleness, foppery in dress and equipage, humbug generally, specifics, nostrums, politics, the pursuit of riches and rich wives—in fine, every occupation, pursuit and evasion have been practiced by many from their studenthood up to old age. “How can he get wisdom that holdeth the plow, and that glorieth in the goad, that driveth oxen and is occupied in their labors, and whose talk is of bullocks,” says the son of Sirach when speaking of physicians. When he reflects upon the variety and accuracy of the knowledge required, how can he conscientiously take under his charge the health and lives of his fellow-men, who has not devoted all his time and energies to the profession. By coveting his neighbors cotton patches and daughters, or by engaging too much in other



occupations to the neglect of his profession, he is guilty of killing, stealing, and often of bearing false witness.

This fundamental rule requires, too, that the physician shall do nothing to dishonor his profession. Yet how much dishonor is cast upon it every day by the pet nostrums, idleness, callousness, ignorance and quarrels of many of the fraternity. Fraternity, indeed! were it not for the respect we owe to it we should compare it to the Happy Family exhibited at the museums. Here is a snarling dog baying a vicious cat upon the bars of the cage — there a malicious monkey is slyly engaged in pulling all the pretty feathers out of the tail of the voluble parrot — here is a canary sick with mortification that its own notes have been imitated and improved upon by a mocking bird — there a lusty game cock, a representative of young America, is striving to dislodge a solemn old owl from his perch; while on the floor a greedy drake gobbles up all the food, crying “quack, quack!”

Many physicians are great sticklers for etiquette, but it is wonderful what crude notions they have of it. They have satisfied their delicacy when they do not call their dear brother “Ass,” “Fool,” “Brute” and “Quack” to his face. Behind his back, they fully satisfy themselves for their restraint in his presence. And even when they do not go so far as to vilify with words, they

“Damn with faint praise, assent with civil leer,  
And without sneering, teach the rest to sneer.”

The envy, jealousy, backbiting, and quarrels of physicians have, to the dishonor of the profession, been the theme of satirists, play writers, and novelists for ages.

But it is not our design to discuss the code of medical ethics, nor to show fully how far either physicians or patients come short of their duties. Let us turn to a more pleasing topic. If we were asked to point out the physician who performs his duties best, and who approaches nearest to what a physician should be in all his relationships — a superior and a friend — we should select the Country Doctor.

The veriest slave toiling in the galleys or in the mines has not a more laborious task than he. Night and day, in the winter's frost and summer's heat, through mud and dust, along the highways and by-ways, through dark swamps and pleasant lanes, he toils on his mission, always intent, always cheerful. There is no pleasure but he must forego it, no obstacle but he must overcome it. By continued use all the powers of his body and mind have become doubly fortified and acute. He has pleasant little plans for keeping cool, and ingenious methods of keeping warm, and admirable devices for taking a nap in his long rides. From a broken buggy shaft to a broken leg, he is always ready, though he has neither blacksmith shop nor apothecary shop at the next corner to appeal to. No emergency can discompose him. Memory is always at her post, and his invention bends even science to his will. Forced to be his own cupper, bleeder and leecher, he pulls off his coat and rolls up his sleeves and goes at it. He blisters and glysters, pulls teeth and gives pills himself to white and black. A sturdy philanthropist, he knows no respect of persons, and will labor and watch all night, equally in the quarter over some decrepit old negro, and in the big house over the master's son. Physician, surgeon, accoucheur, dentist, apothecary and nurse, he travels

along independently with his saddle-bags and case of instruments, forced to make the whole science of medicine his Speciality. Who would be a country Doctor?

But there is another side to the picture. As he plys his toilsome way, there is not a negro he meets but has a ready bow and grin for him whom he looks upon as akin to the Gods, as *his* doctor and special friend. At his destination he is met by the warmest kindness and the deepest respect; his very presence has a charm which brings relief. He attends now perhaps in the families of those at whose birth he assisted, and who from their childhood have loved and had faith in him. As the first friend of the family, many are the family secrets which have been confided to him, many the anxious private consultations with him by fond mothers and doting husbands. He knows the people and studies their very souls, and he has a place in their hearts and cares little for the malicious jealousy of rivals. In his circuit everybody knows everybody, and he has the health of all in his charge, so that he is the cynosure of all eyes, and good wishes and kind attentions accompany him everywhere. One has to tell of a mother's life he has saved, another of a wife's, another of his own; one tells his excellence as a surgeon, another lauds him as a nurse, while a third speaks of his integrity as a man; and blessings and praises are bestowed upon him from all sides. Blessings on the country doctor say we too. Raised in the country ourself, from our earliest childhood we have had for his character the highest veneration, and for his person the deepest love. He is the hardest worked, the most faithful and the least understood of all men in the high resorts of science; and we picture his character and his labors not only to vindicate him, but also that those who are preparing in these resorts for practice in the country may know what they must become, and what they must undergo, to fill his place.

We do not wish to be considered as alluding either to any particular individual, or to the whole class of country doctors, but only to those (and they are many) who come up to our description. Nor do we wish to detract from the high position of city physicians. Their spheres are different and each may fill his sphere perfectly. We commenced to speak of the relationship of physician and patient, and we think that all the duties of that relationship are better defined, and therefore better filled, in the country than in the city. For that reason we have selected the country doctor as our model. The city physician has neither his hardships to undergo, nor his pleasures to repay him. He may attend for years in some families, and may gain much love and feel much of that satisfaction of the affections which such a relationship produces; but the largest class of persons who fall under our care is very, sadly different. He can generally consult his comfort, and he has every convenience of his profession at hand. Besides this, he can confine himself to any one branch of his science, which the country doctor cannot do.

We have chosen the subject of medical ethics, and particularly the relationship of physician and patient, as eminently proper for this first number of our new volume, and it is so suggestive that we hardly know how to end it. Let us in conclusion beg our younger brethren to pause and reflect upon the great responsi-

bilities of their position and the great knowledge and high principles and conduct it requires, and in view of these, "to give their heart, soul, mind and strength to their profession and do nothing to dishonor it." So exalted is our opinion of American intellect, of the powers of individual application and unanimity of effort, and of the hygienic influence of medical faith — that we believe they can in this way elevate the science to a height, and give to it a virtue never known before. And while it shall be our aim to contribute to the diffusion of science, we shall equally strive to raise the *morale* of our profession to a level with our model Country Doctor.

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The annual report of the "Société Anatomique de Paris," has just been published. For the last twenty-nine years, the rarest as well as the most instructive specimens of pathological anatomy have been laid and discussed before this learned Society, and as it still continues to be the recipient of all the interesting specimens which the hospitals of Paris daily furnish, we have imagined that an extract from its last annual bulletin, would be interesting to our readers.

Of the many interesting facts which it contains, we will first mention two anomalies, occurring in the nerves of the face; these are the more interesting, as that subject has been as yet but little studied. The first of the above specimens, consists in an anomaly of the *buccal nerve*, found by M. Galliet, in a child four years of age. This nerve instead of arising from the external trunk of the inferior maxillary, was given off directly from the ganglion of Gasser, just between the superior and inferior maxillary nerve, and was perfectly disconnected from the anterior or smaller root of the *trigeminus*; after its exit from the cranium, it resumed its natural course. This fact goes to prove the opinion, that the *buccal nerve* is a nerve of sensation. M. Galliet, demonstrated in the same subject the mylo-hyoidean, (branch of inferior dental,) three or four times more voluminous than natural, giving rise to an anastomotic filament, which joined the lingual nerve. This last phenomenon is constant according to *Sappey*; but it is excessively rare to find it of such dimensions.

M. Titon, presented an interesting case of total absence of the bladder, in a full grown child, which died five days after its birth. On the inferior portion and lateral borders of the penis, are seen two small orifices, which answer to the openings of the ureters. Upon the superior surface, the two corpora cavernosa are separated by a furrow; the two *ossa pubes* disconnected; the ureters, of the size of a quill, converge and terminate in the two openings mentioned above. The bladder was missing.

Quite a large number of specimens of fractures have been presented to the Society, of which, we will only mention a few. The first worthy of notice, is a fracture of the *seventh cervical vertebra*, caused by a forced flexion of the head forwards. Death occurred on the third day, and was owing to paralytic asphyxia; besides the osseous lesion dissection revealed an effusion into the spinal marrow, opposite the fracture. This fact is interesting, as regards the mechanism of the lesion, for the great Boyer had positively denied the possibility of fractures of the cervical vertebra, occurring by a forced flexion of the head.

In a second case of direct fracture of the seventh vertebra, related by M. Gaujot, constant erections of the penis were observed. This symptom then, cannot be considered as indicative solely of a lesion of the cerebellum. To this case, we may add another, that of a man, who came in the Charity Hospital of this city with a fracture of the third and fourth cervical vertebræ. In this case, priapism was constant. A case of fracture of the neck of the femur, is worthy of notice; the lesion extended from the anterior border of the great trochanter, to the linea aspera, and the neck had been driven into the great trochanter: the complete inversion of the limb, was undoubtedly owing to this penetration. This fact had already been mentioned by Desault, Dupuytren, A. Cooper, Sanson and Velpeau.

Several specimens intended to show the different stages of *osteomyelitis*, were presented, and three distinct stages established; that of congestion, that of plastic exudation, which may contribute to the obliteration of the medullary canal, and last, that of suppuration, sometimes mistaken for osseous tubercles.

M. Rouget presented a case of alteration of the osseous tissue, as rare as it is interesting; it consists in a deposit of a greyish and foetid fatty matter beneath a sequestrum of the frontal bone, and also in the cavity of the frontal sinus, composed of fatty granulations, amorphous grey granulations, and numerous crystals of stearic acid, free and isolated. This singular substance, the origin of which is unknown, has also been noticed by Messrs Follin & Gosselin, beneath sequestra of the cranium, and Robin found it once in a diseased tibia.

A few interesting cases of dislocations could be mentioned. Mr. Bidart showed two cases of dislocations of the humerus; in one case, reduction was effected on the fifth day, and the patient died twenty-seven days afterwards, from the effects of a fracture of the neck of the femur, which occurred at the same time. A post mortem examination revealed a destruction of the anterior border of the glenoid cavity, and a loss of substance corresponding to the posterior border of the os humeri. The laceration of the capsule, was completely cicatrized after twenty-seven days. M. Rombeau presented a case of simple dislocation of the humerus, the reduction of which, was followed by arthritis and suppuration. These cases of suppuration occurring after reductions are comparatively rare; they have generally been known to occur in the coxo-femoral articulation.

We should not omit to mention two cases of dislocation of the dorso-lumbar vertebræ, without fractures presented by M. Landry; in one case, the fifth dorsal vertebra was dislocated upon the sixth; and in the other, the twelfth dorsal was similarly displaced upon the first lumbar.

We will finish the analysis of this truly interesting annual report in our next number.

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**NEW MOUTH WASH FOR MERCURIAL STOMATITIS.**—Dr. Norman Cheerers (Indian Annals of Medical Science, April, 1854) has published several cases of salivation, treated by a gargle of two to five drachms of the compound tincture of iodine to eight ounces of water, in which the curative effects of the remedy are made quite evident. We have ourselves tried it in two cases with marked benefit.



## CORRESPONDENCE.

PARIS, January 24th, 1855.

My recent trip to visit the German and Italian schools of medicine has prevented my sending you a monthly summary of the news transpiring in the medical world of Paris as I had promised. I have just returned and write you this as an earnest of my future punctuality.

Our faculty of Paris has just met with a severe loss in the death of its professor of internal pathology, M. Requin, who died in the beginning of the present month of a disease which presented many though not all of the symptoms of typhoid fever. His physicians, Messrs. Chomel and Grissolle, at first supposed it to be a fever of this character, but soon one of the inferior extremities becoming swollen, M. Nelaton was called in who believed it to be owing to an abscess situated in the pelvis; from this, purulent infection is supposed to have taken place, but no post-mortem examination was made.

There are already many candidates for the professorship thus made vacant, among the most prominent of whom may be mentioned Natalis Guillot, of Necker Hospital, M. Monneret, his colleague, and M. Beau, of Cochin Hospital; as the nomination no longer depends upon success in a *concour*, but is made by the head of the government, no one can tell who will be the fortunate candidate. The names given above are already well known to the medical public. Two other vacancies caused by the same death, one of physician to the Hôtel Dieu and the other of membership of the Academy of Medicine in the section of pathological anatomy, are to be filled—the names of the candidates I will give you in my next letter. M. Requin was first an *agrégé* of the faculty, afterwards the *chef de clinique* of M. Chomel, and in 1851 was named professor to fill a vacancy caused by the permutation of M. Piorry from the chair of clinical pathology to that of practice of medicine, which latter had been filled up to the time of his death by M. Fouquier. M. Requin was the author of several memoirs and a treatise on internal pathology, in which he displays much erudition; he also assisted M. Chomel in writing one of the volumes of his *Clinique*.

The Academy of Medicine has for several months been occupied in discussing the value and utility of the microscope as an aid to diagnosis; it is not yet at an end, and seems carried on in an improper spirit since microscopy has no representative in the Academy to appear in its defence. Outside of its halls this branch of science numbers among its supporters the well known names of Lebert and Robin; Broca appearing for it in the *Moniteur des Hôpitaux*, Verneuil in the *Gazette Hebdomadaire*, and Follin in the *Archives de Médecine*. Thus we see represented the most brilliant intellects of the school of young surgery, among whom three embryo professors may be designated, favoring doctrines adverse to those received by the oldest members of the profession. Velpeau is most bitter in his attacks upon the use of the microscope, stating that he placed more reliance in the diagnosis of tumor based upon its general characters and appearance to the naked eye, than in the pretended presence of the so called characteristic cancer

cell (for the discussion seems simply to be upon the capability of microscopists to diagnose cancer.) On this subject M. Malgaigne delivered himself of a speech far more brilliant than solid, and, according to his habit, attacked the combattants on either side, telling Velpeau that for his own part he had learned much by microscopy, and he would be ashamed to advocate now the same classification of tumors which ten years ago he had defended in a memorable discussion before the Academy; disapproving, at the same time, of microscopy, for not making a line of demarcation between the practical and scientific.

M. Robert, surgeon to Beaujon Hospital, was the only one who took a decided stand in favor of the instrument, but it is bruited about that this was caused more by rancor towards Velpeau, dating back to the time when he appeared, with the latter for his judge, at a *concour*, than to any heart-felt conviction.

M. Larry's discourse was as cold and as precise as his manners.

The veterinary surgeons, Messrs. Delafond and Leblanc, now stepped forward and showed themselves better acquainted with the subject than those who had spoken before them.

M. Barth, so well known in your country, made the general impression both in and out of the Academy, that had he been born fifteen years later he would have been a microscopist to the very bone.

I have always thought that as no member of the Academy was a practical microscopist, as a body the Academy was an improper tribunal to judge the question.

Broca and Verneuil have written most excellent articles in defence of the position they sustain. The Academy will make this question the subject of a *concour* for a prize to be given in two years from the present time, but I doubt if our distinguished microscopists will take part in it.

I can also send you some little news about Hôtel Dieu. Jobert de Lamballe, as you know, is now lecturing in an amphitheatre which was built expressly for him. His lectures used to be but short remarks upon the cases in his ward, now they are long and full of learning. The medical world was so little prepared for this, that it has received without hesitation almost, the scandalous report of their being prepared by a younger and more active brain. M. Nelaton, of the *Clinique*, has made his lectures on clinical surgery so attractive, that the celebrated surgeon of the Charity, now lectures to emptier benches, than has been his wont for long years gone by.

Jobert and J. Cloquet, are candidates for Lallemand's seat in the Academy of Science. The former, it is reported, stands the best chance. Cloquet has not been much before the public for some time past, but I have been informed that he intends reopening his course this summer, *qui vivra vera!*

Yours, etc.

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LOUISIANA STATE MEDICAL SOCIETY—*Annual Session, February 5, 1855.*—The Society met this day at half past 12 o'clock, in the Medical College, Dr. E. H. Barton, President, in the Chair.

The annual address was read by the President, after which the following report from the Board of Administrators was read to the meeting:

The Board of Administrators has the honor to announce that it has had published, in pamphlet form, the proceedings of the last year. It has also published, under the instructions of the Society, two hundred copies of its constitution and by-laws for distribution among its members. It has also the gratification to add that application has been made to the Society by members of the profession in Natchitoches to become tributary to this Society, under the style and title of the "Natchitoches Medico-Chirurgical Society," which awaits approbation of the Louisiana State Medical Society.

The Society then adjourned until the following day.

*February 6, 1855.*—The Society met this day, Dr. E. H. Barton, President, in the Chair. The minutes of the previous day's proceedings having been read and the roll of members called, Dr. Picton nominated as President of the Society for the present term, DR. WARREN STONE, who was unanimously elected.

The following gentlemen were then elected Vice Presidents, viz: — DRS. J. M. W. PICTON and S. A. CARTWRIGHT, of New Orleans; DR. EUGENE PALMER, Parish of St. James; MR. McLEOD, of Thibodeaux, and DR. J. GILPIN, Shreveport.

The rules, on motion, were dispensed with, and the following gentlemen elected to their respective offices by acclamation.

*Corresponding Secretary*.....DR. J. D. FOSTER,  
*Recording Secretary*.....DR. J. L. CRAWCOUR,  
*Treasurer*.....DR. E. D. FENNER,

Dr. Barton then vacated the Chair, having called Dr. Picton, Vice President, to preside.

The Standing Committees being called upon to report, Dr. Picton, as chairman on the subject of "Medical Education," made some verbal remarks on the subject and concluded by reading at some length, extracts from Dr. Cabel's report, published in the last volume of the transactions of the American Medical Association.

On motion of Dr. Fenner, it was resolved and carried, that Dr. S. A. Cartwright be invited to address the Society on Thursday next, the 8th instant, on the subject of yellow fever at Natchez, in all its bearings, and to offer any remarks he may have to make concerning yellow fever and quarantine in New Orleans.

The Society then adjourned until the next day.

*February, 7.*—The Society met this day, Dr. Stone, President, in the Chair. The President addressed the meeting, thanking them for the honor they had done him in calling him to preside over the Society.

After the roll of members had been called, the reports of the Standing Committees were called for. Dr. Crawcour, chairman of the Committee on Practical Medicine read the report on that subject.

The report of the Treasurer was then read. The Society then adjourned until the next day.

*February 8.*—The minutes of the preceding day having been read and the roll called, the report of the Board of Administrators was read, which having reported favorably of Drs. Grier, Anfoux and Holmes, these gentlemen were unanimously elected members of the Society.

Dr. Cartwright, having been called for agreeably to the order of the day, addressed the meeting on the subject of yellow fever. After the reading of the paper, it was moved and carried, that the thanks of the Society be tendered to Dr. Cartwright, for the able paper he had read before it and that a copy be requested from him to be placed in the archives of the Society.

The meeting then adjourned to the next day.

*February 9.*—The Society met this day at half past 12 o'clock, Dr. Picton, Vice President, in the Chair.

The minutes being read and the roll of members called, the name of Dr. W. B. Dodson was reported on favorably by the Board of Administrators. Dr. Dodson was then elected.

It was moved by Dr. Fenner, and carried, that the assessment for the ensuing year be fixed at two dollars.

After a vote of thanks to Dr. Picton, the Society adjourned to meet in the same place on the second Monday in March next, at half past 12 o'clock.

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We make the following extract from the record of the Physico-Medical Society of this city, in as much as it contains facts which will be interesting to the profession; the evidence adduced in favor of the use of the sulphate of quinine, as administered in the Charity Hospital in yellow fever, is so conclusive and so reliable, that it cannot fail to weaken the prejudice, which exists against it in the minds of many excellent practitioners.

“Dr. Hunt, after adverting to late discussion on fatty degeneration of the liver, and giving his views on that subject, and the general pathology of yellow fever said, that he had stated at a previous meeting of the Society, when advocating the treatment of yellow fever, by the use of quinine and opium, that he was confident that of the patients admitted into the Charity Hospital during the late epidemic, and during the expiration of the first twenty-four hours of the attack, not one in fifteen had died; he was now prepared to show, by the best authority, that not one in twenty died of those who entered within the specified time.

On an examination of the books of the hospital, he had found that there had been 565 admittances of patients of yellow fever, who entered within the first twenty-four hours; only 27 of whom had died. Of these, fully fifteen-sixteenths had been first seen by the house surgeon, Dr. Choppin, or his assistant, Dr. Canter, whose almost invariable custom, was to administer the haustus quiniæ of the house.” This, as it is well known, consists essentially of twenty grains of quinine, and forty drops of laudanum.



Dr. Ebden, in the *Indian Annals of Medical Science*, makes very favorable mention of the use of the hydrochlorate of ammonia in neuralgia. He notices cases in which its action has been very beneficial, such as *ticdouloureux*, tooth-ache, *clavus hystericus* and even in one case of neuralgic dysmenorrhœa. He prescribes the salt in doses of twenty to thirty grains in mint water or camphor mixture to be taken every twenty minutes for three doses.

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#### CHARITY HOSPITAL.

In the surgical wards there have been several cases offering much that is interesting. One, that of gluteal aneurism, for which ligature of the gluteal artery was performed, is reported in full in this number of the journal.

A woman was brought to the hospital, concerning whose history but little could at first be learned, as she spoke only German; upon examination a purulent collection under the scalp was discovered, situated on the posterior and lateral portion of the head. This was opened and a large quantity of pus escaped. On examination of the wound a fracture of the skull was discovered of considerable extent, and a portion of bone corresponding to the size of the fracture had entirely disappeared. A considerable quantity of softened cerebral matter escaped at the opening and a quantity of the brain protruded. It was afterwards found out that she had been struck upon the head by a large blunt billet of wood, and it is supposed that the missing portion of bone had slipped under and applied itself intimately to the inner tablet of the skull. The woman is still alive and doing pretty well.

There were lately two operations for the removal of tumors of the lip. In one case where the degeneration was extensive, two incisions were made parallel to each other, extending from the angles of the mouth to the *os hyoides*; this flap was then dissected loose, the diseased mass removed, the flap slid upwards and retained in position by the twisted suture. The operation has succeeded beautifully, and but little deformity is to be seen. The tumor proved to be an epithelioma when, examined under the microscope.

In the second case the disease was not so extensive, being confined to the angle of the mouth. This was removed by a V incision; the result has been very satisfactory.

In the eye wards a case of cataract may be mentioned as presenting some interest. The patient had lost one eye by previous inflammation and was affected with hard lenticular cataract of the other, accompanied with softening of the vitreous humor (*synchysis*.) The case was unfavorable, but it was determined to operate. The lens was accordingly depressed by the sclerotical puncture, in the usual manner, and remained in its new position after the removal of the needle from the eye. The second day on examination it was found occupying its old place, of course obstructing sight. As soon as the inflammation had subsided, the operation was performed again, but with a like result. After the expiration of a week, depression was made for the third time, and on this occasion with success. The patient now, three weeks after the operation, sees well, and there is no sign of the opaque lens to be observed.

In one of the medical wards two cases of double pneumonia, well characterized, have recovered under the use of mercury, pushed in one case to salivation, and tartar emetic in large doses, eight grains in the twenty-four hours, and blisters; the patients were not bled.

Two interesting post-mortems were recently made; one, in which together with an aneurism of the ascending aorta. The heart was found diseased in all its parts. The semilunar valves of the aorta and pulmonary artery were unusually thickened; the latter together with the tricuspid and mitral valves studded with the largest vegetations; these were also found attached to the fleshy columns. The right ventricle was much enlarged, and its walls, as well as those of the left, enormously thickened. The post-mortem was interesting inasmuch, as the one specimen presented nearly all the lesions observed in diseases of the heart.

The second post-mortem was of a patient who entered the hospital with bilious colic, after recovering from which he one morning suddenly died. On examination an aneurism of the size of a hen egg was found, of the coelic axis, the rupture of which had caused his death.

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CHARITY HOSPITAL REPORT—Month of February, 1855:

Admitted, - - - - -	- - - - -	744
Discharged, - - - - -	- - - - -	711
Died, - - - - -	- - - - -	122
Births, 10—5 males, 3, females, and 2 still born.		

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We have received, through Thomas L. White, Canal street, a small volume entitled "What to observe at the bed side and after death in Medical Cases," and most heartily recommend it to the notice of medical students.

Also, from Dr. A. Mercier, the author, a pamphlet containing his address delivered before the Physico-Medical Society of New Orleans, at its last anniversary meeting.

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Our Journal has now such a list of subscribers, as to make it a valuable medium of advertising to Druggists, Surgical Instrument makers, Bandagists, and others, whose occupations either remotely or directly are connected with the profession. We therefore place before them the terms of the *New Orleans Medical News and Hospital Gazette*, and solicit their patronage.

One page.....	\$7 00
Each additional insertion.....	5 00
One-half page.....	5 00
Each additional insertion.....	3 00
One-fourth of a page.....	3 00
Each additional insertion.....	2 00

*Advertisements in Double Columns.*

One square,.....	\$2 00	One-half square,.....	\$1 50
Each additional insertion,.	1 50	Each additional insertion,	1 00

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A complete index to the first volume of the *Journal*, together with a title page will be found accompanying this number.

NEW ORLEANS

MEDICAL NEWS AND HOSPITAL GAZETTE.

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VOL. II.

APRIL 1, 1855.

NO. II.

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The writings of the late Dr. John Harrison, on yellow fever, have become exceedingly rare and difficult to procure; their universally acknowledged excellence, and the desire of many to possess them, is sufficient reason for their republication.

We copy from the New Orleans Medical and Surgical Journal, Vol. 2, No. 2, for the year 1845.

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Remarks on Yellow Fever.

By JOHN HARRISON, M. D.,

*Professor of Physiology and Pathology in the Medical College of Louisiana.*

To those at a distance, the mention of New Orleans calls up the idea of disease and death as inseparable associations; yet, during eight months in the year there is not, perhaps, a healthier city in the Union. In the four remaining months we are liable to suffer from the yellow fever—the pestilence of the South—the great obstacle to increase of population in her cities, and, of course, to all other advantages which increase of population brings with it. Hence the causes of this disease, its pathology, and the methods of treating it, become of immense importance to the public, and hence, too, a duty rests upon those who have seen much of the disease, to impart their experience.

I purpose in the following pages to state the symptoms of yellow fever, and, as far as practicable, the lesions found after death; to sift, as far as possible, its pathology, and to discuss the best methods of treating it. In so doing, I shall confine myself to my own convictions and experience without reference to the writings and opinions of others. What I write

is the result of observations made during thirteen years' practice in the city of New Orleans; during ten of which I was connected with the Charity Hospital, either as house-surgeon or visiting physician. In that noble retreat for the unfortunate, and in private practice, I have treated many cases of the disease, and have made or assisted at several hundred *post mortem* examinations. In addition, I have experienced the disease in my own person. I make these remarks for the purpose of showing what opportunities I have enjoyed of studying the disease; and if, in the present paper, there be found little that is new or of much value in a positive sense, it still may, I hope, be of some service in destroying erroneous notions of the disease, contracted by those who have never seen it.

In a paper like this, it is necessary to be brief. Yellow fever, like all other diseases, has its specific symptoms, developed in its rise, course, and terminations, and by these we designate the disease. But the law of variety prevails in disease as well as in organology, and in none more than in yellow fever; and as we cannot expect to find two faces or two leaves exactly alike, we may also expect to meet with varieties in cases of diseases arising from the same cause. The most important varieties in the symptoms of yellow fever will be mentioned, but the minute detail of cases avoided; inasmuch as it is only from the essential characters of the disease, sifted from accidental or unimportant occurrences, that we can expect any general and practical good.

The yellow fever makes its earliest appearance about the latter days of July, and continues until the appearance of frost, which is usually about the middle of November. Frequently, however, it appears later in the season, and sometimes disappears, at least as an epidemic, before frost—probably for want of fuel. A few cases are also met with some weeks after cold weather has set in; but the disease invariably ceases as an epidemic after a frost sufficiently severe to kill the leaves of trees and annual plants. Many attempts have been made to connect its appearance with meteorological phenomena, but all, so far, have been unsuccessful. I have observed that in those years in which epidemics prevailed to a greater extent than usual, they were preceded by intermittents of a bad type. These last prevailed during the latter part of May, in June, and the earlier part of July. Whether they are the constant forerunners of yellow fever or not, can only be decided by a long series of observation. It attacks only strangers, those born in the city being perfectly exempt from the disease, though it is still a question whether they do not pass through it in infancy. The creoles of the State, residing out of the city, and never subjected to the disease, are as liable to attacks of as grave a character as



those born farther North. Upon recovery, the citizen is said to be *acclimated*, and enjoys an immunity from the disease; but this, though general, is not universal, for I have known several who have been attacked a second time. I have never known, however, such cases to terminate fatally. Some persons, also, (and those not a very few) pass through the most violent epidemics without being attacked at all, and are then considered fully acclimated. This rule, however, does not apply to those who have remained in the city during what are termed *mild* epidemics; in other words, those who pass through such epidemics as occurred in 1833, 1837, 1839, and 1841, without an attack, are considered as safe as those who have recovered—otherwise not. Persons coming from cities where the disease prevails, and in this respect similarly situated to New Orleans, also enjoy an immunity from the disease. Such is Charleston, S. C. Again, the susceptibility to the disease seems to be renewed by long exposure to cold weather, as young men born in this city, but sent to Europe or our Northern colleges, are sometimes attacked on their return. Negroes certainly have the disease in a much milder form than the whites, and the mortality is far less among them. I cannot say that I have observed any difference as regards the sexes, either as to the number or the malignity of the cases. As a general rule, children have it milder than adults.

From the preceding remarks it will be seen that the yellow fever is by no means regular, in its visits, nor are those visits of equal malignity. Of the years in which I have observed it, the following is a summary:

- 1832, Epidemic.
- 1833, Violent epidemic.
- 1834, Epidemic.
- 1835, Mild epidemic.
- 1836, Very few cases.
- 1837, Violent epidemic.
- 1838, Few cases.
- 1839, Violent epidemic.
- 1840, None.
- 1841, Violent epidemic.
- 1842, Epidemic.
- 1843, Epidemic.
- 1844, Mild epidemic.

The terms “mild epidemic,” “epidemic,” and “violent epidemic,” are intended to express degrees, both as to the prevalence and malignity of the disease.

In 1832 a violent epidemic of Asiatic cholera raged at the same time the fever prevailed. In 1833, 1834 and 1835, there also existed sporadic cases of cholera.

When I add that the fever is confined to the city and neighboring towns, or to those who enter them, but afterwards return to the country; that persons residing in the country around, even though they be unacclimated, are safe from its attack, and that, in all years in which it appears, we see very mild as well as very malignant cases, I have mentioned, I believe, the principal points connected with the appearance of yellow fever.

Let us now examine the symptoms.

*Symptoms.*—Omitting individual peculiarities, let us sum up those symptoms by which the disease is recognized. We will suppose a person who has been protected, in the best way possible, from those obvious causes of disease which may affect the health at any season. He is well lodged and clothed; he is temperate in his diet, and is careful not to expose himself to the sun, to wet weather, or the night air; he is abstemious with regard to alcoholic liquors. These precautions, however, avail him little. In the midst of excellent health, he is stricken down. He experiences a rigor, which sometimes ends in a violent ague; in a few hours a burning fever comes on, with distressing pains in the head, back and limbs. The tongue, however, is as yet moist, and the urinary secretions copious; but the eyes are generally dull and heavy, and intolerent of light.

In the course of twenty-four or thirty-six hours, the usual consequences of violent fevers ensue; the secretions are diminished in quantity, and altered; the tongue becomes red around the edges, pointed and furred, with a white or yellowish down in the middle; sometimes, though rarely, it is dry. Sordes appear upon the teeth. The urine is highly colored, and in many cases highly corrosive. The skin is usually moist, with sudamina scattered here and there—principally over the breast. It is, however, sometimes dry, and very hot. The pulse continues strong and quick, beating at the rate of 108 to 120, or over, per minute.

Towards the close of the third day, or beginning of the fourth, the fever intermits. The prostration of muscular power, which has been increasing from the first moment, is now complete—the patient being scarcely able to turn in his bed. The pulse falls in frequency even below the natural standard, though in general retaining its usual fullness. The stomach now becomes more or less irritable, being unable, in most cases, to bear even a spoonful of cold water. The skin and eyes assume a yellow tinge, and both are highly injected. This injection, however, does not appear to be

attended with high action, for the skin is now rather cold to the touch and the secretions from it seem altogether to have ceased. If we press with a finger upon the surface of the body, we observe upon removing it, a white spot, which slowly and gradually resumes its former color. This is strikingly in contrast to the quick flash wherewith the blood returns into the tissues on the first or second day. This injection in truth is of a passive character, and is undoubtedly one of the consequences of the foregoing violent actions to which the whole system has been subjected, and by which the organization of the tissues have suffered. In short, the parts are changed in structure—have lost, in consequence, their natural elasticity—make little resistance to the blood coming from the heart,—and are injected as we might inject a sponge with a syringe.

From the condition last described, the patient gradually returns to health or dies. If death is to be the result, we shall see the irritability of the stomach growing almost hourly greater—even a teaspoonful of cold water being thrown up the moment after being swallowed. An indescribable *malaise* afflicts the sufferer, although he appears at the same time to be without any fixed or local pain. A continual sighing, involuntary groans, extraordinary restlessness, great diminution, or a total stoppage of all the secretions, announce the approach of the fatal symptom—black vomit. On the fourth, fifth or sixth day, this is thrown up, and death soon closes the scene.

The matter first thrown up consists almost entirely of the drinks taken. A few flocculi of mucus may be discerned floating here and there in the liquid. Towards the approach of black vomit these flocculi increase in quantity, and are of a deep gray color. Mixed with them we may often find, upon a close examination, a few striæ of a darker matter—in other words, of black vomit.

This last mentioned fluid is not thrown up in the manner that emesis usually occurs. The muscular motions, and the sounds accompanying the ejection, are peculiar. There is no violent retching; a sound is heard, caused apparently by a hiccough mingled with a cough, and the black matter is ejected. In many cases, this is done so violently as to send it many yards. I have seen it, in the Hospital, thrown entirely over the bed of the next patient and fall on that adjoining.

The conditions of the patients when throwing up black vomit, vary most remarkably. Some are quiet—answer questions—and appear rational, but indifferent to their fate; so much so, that they will frequently respond to questions concerning their condition by saying “they have the black vomit.”

Some will even get out of bed and walk about—declare they are perfectly well, and wish to dress themselves. I have seen this occur, and death take place in half an hour afterwards. Others are delirious, and force is required to keep them in bed; others lie in a semi-comatose state, and keep up a constant and most distressing moaning.

Such is the usual course of the disease; but there are a vast number of individual difference which we ought to expect, since it would be difficult to find any two persons in precisely the same condition at the moment of attack; and, therefore, it is but in the application of the well known law “that the same cause acting on different subjects must produce different effects,” that we should be led to expect individual differences in all epidemic diseases. Let us take notice of some of the most remarkable.

A. The symptoms of yellow fever will vary according to all those general circumstances which modify those of other diseases. Thus, we may have the congestive, inflammatory, or typhoid form, according to circumstances. In many cases we have gastric irritability from the very commencement, together with pain or pressure at the epigastrium. Towards the close, we have, in many cases, passive hæmorrhages from the application of cups, from leech bites, from wounds given by the lancet in bleeding, etc. They also occur from the gums and tongue, from the nostrils, from the bowels, from the scrotum, from the eyes, (though rarely,) and sometimes from the ears. These hæmorrhages, though dangerous, are not, however, indicative of a desperate case, since I have known many of them to recover. In one case under my charge, the patient bled for three days from the gums and tongue, losing about a pint a day, when it was arrested by the use of kreosote, and she recovered. The blood lost coagulated to some extent, but very imperfectly.

B. In some few cases there is no actual chill at the commencement of the disease; and indeed, no fever ever makes its appearance. These cases are characterized by extreme restlessness from the beginning, and by an indomitable disposition to walk about; hence, they are sometimes called “walking cases.” They present, as is well known, by far the most formidable variety to the physician, and indeed are geneally regarded as incurable. The patient, in this form of the disease, presents a natural eye, tongue and pulse. The skin is also natural, except that towards the extremities it becomes cold, and very often the hands and feet look as if they had been subjected to the long action of water. When the patient is questioned he seems loath to answer—returns sullen replies, and tells the physician there is nothing the matter with him. There is no gastric irritability, and



no pain or pressure at the epigastrium. The stools are fluid, and the urine copious and limpid. In all this absence of the usual symptoms which mark the disease, there is, however, a very peculiar and characteristic expression of the countenance, which cannot be described, but when once seen is never forgotten.

The condition above described, continues until the end of the second or beginning of the third day, when a change occurs. The patient lies down from inability any longer to keep up; the pulse sinks in volume, but increases wonderfully in frequency; the skin grows hot; the stomach swells up, protruding the walls of the abdomen before it; black vomit in large quantities is thrown up at the first gush, and death follows very shortly afterwards. This variety is exceedingly rare.

C. In other cases, we have all that tribe of symptoms which characterize, ataxic fevers; such as, great irritability in the capillary circulation; certain parts becoming suddenly cold and palid, and the next moment red and hot; copious perspiration, alternate with hot and dry skin; the bowels are now loose, and a few hours afterwards the patient may be suffering from the torments of constipation; the respiration is hurried after the slightest exertion; the urine, in the morning limpid, and in the evening perhaps highly colored; the tongue may be this moment moist, and in a short time, without appreciable cause, perfectly dry; chills and rigors supervene from the slightest application of cold; tremors, nervous delirium, etc., also occur in this form of the disease. It is generally fatal.

D. Towards the close of the attack, the brain frequently becomes affected. The physician may have flattered himself that his patient is perfectly safe; the fever has subsided for twenty-four hours or more; the tongue and pulse are good; there is no gastric irritability of any consequence; urine has recently been passed; all things, in short, tend to convince him that the case will end in a speedy cure. After a while, some slight remark from the patient arrests his attention; the nurse informs him that she thinks the patient has been "wandering in his mind;" upon further questioning and trial, however, he can discover nothing more. There is, however, an unquiet glance of the eye which still further excites his suspicions. He pays a visit earlier than usual and enters the sick chamber with a foreboding heart. He finds his worst fears realized—nervous delirium has set in, and he abandons all hope.

In several cases I have seen the brain seriously affected in the first few hours after the attack. In some, convulsions occurred, ending in coma, from which last they never recovered. These cases, however, were in children.

In other cases, though there were no convulsions or coma, the brain was attacked in the beginning, and continued affected throughout the disease. These cases are not characterized by *delirium*, properly speaking. The little tricks—the perverse pleasure of thwarting their physician and nurses—the great delight shown upon the success of their schemes, characterize the mental perversion of the insane, rather than of the delirious. These cases, though not altogether hopeless, are exceedingly dangerous.

Delirium often comes on during the first or second day. It is generally relieved by depletive means, and is not considered a very dangerous symptom.

I may here relate a case which occurred in my practice in 1837, and which presents some singular features. The patient, a robust young man, about twenty-five years of age, had gone through a severe attack of fever, and on the sixth day was pronounced safe. Thinking himself so, he desired his nurse to bring him a mint-julep from the bar of the hotel in which he lay. On her refusing, he insisted that the physician had ordered it. It was brought and drunk. The next day I found him perfectly insane, but without fever or any physical symptom of disease. His insanity manifested itself by loud talking—wild, but somewhat coherent fancies, hysterical laughter and tears; paroxysms of rage, succeeded by whining complaints of ill treatment; insomnolency, and odd remarks, that provoked to laughter the most saturnine of his attendants. Under the use of opiates and the shower bath, he recovered, and is still living in good health; but it was more than two months before he was completely relieved.

Another affection of the encephalic organs is manifested by an unusual disposition to sleep. It appears soon after the patient is taken down. There does not appear to be so much suffering as in ordinary cases; the pains in the head, back and limbs, are by no means so violent. The pulse is frequent and quick, but small and weak. The skin is usually pale, or rather, sallow. But the most formidable symptom is the tendency to sleep. It at once alarms the nurse and attendants; and it may well do so. The patient may be easily roused, will answer questions put to him, but while talking falls away into slumber. These sleeping cases, as they are called, are justly considered as being among the most desperate the physician can meet with.

There is another phenomenon which is of very general occurrence when the case is about to terminate fatally. I allude to an exquisite tenderness of the epigastric region, supervening between the fourth and sixth days. The slightest attempt to press upon the parts is resisted by the patient with

all the expressions of intense agony and horror. What is the cause of this extraordinary sensitiveness? Is it inflammation? Assuredly post mortem examinations flatly contradict such a supposition. It is known to exist most intensely in cases where, after death, we find but very trivial lesions. Besides, it is not experienced in other diseases, in which, after death, we find much greater lesions of the stomach and duodenum. Nor does this phenomenon resemble that produced by pressure at the epigastrium in cases of simple gastro-enteritis. It rather resembles the effects produced by pressure, in cases of violent peritonitis. But peritonitis does not exist. I have never met with decided peritoneal inflammation in all the autopsies of yellow fever that I have witnessed. Besides, this exquisite tenderness is not confined to the epigastrium. It is experienced all over the abdomen and even in the limbs, but is certainly most severe over the stomach.

It appears to me that this morbid sensibility must be referred to a condition of the nervous substance, induced by the preceding violent actions to which the whole system has been subjected. The tissues have been changed in their constitution; not only as regards the minor points of structure, but also chemically. The nervous substance has suffered in common with the rest, and hence the effect. In short, the exquisite pain is owing to a morbid condition of the nervous substance, not to increased action in the parts. It is a most fatal symptom.

Another phenomenon which seems related to that of passive hæmorrhage, is the appearance of petechiæ. They never appear until the febrile symptoms subside, and seem in truth, to be nothing more than small spots of blood congested in the tissues. Musquito bites also become dark and livid, and much resemble petechiæ. They are both outward signs of that complete prostration of the powers of life, that torpor and want of action, in which the system is left after the preceding violent actions have subsided.

Another and most fatal symptom frequently appears towards the close of the attack. It is a total suppression of the urinary secretion. The kidneys appear to have been completely disorganized by the foregoing violent actions.

There are also some minor varieties of symptoms which it may be worth while to take notice of, and which I shall here group together. The bowels are sometimes costive and with difficulty operated on, while in other cases, or at other periods, they are extremely relaxed. The dejections also differ exceedingly in appearance. At times they are exceedingly large and offensive; at others, they are watery and slightly tinged with coloring matter.

Their color, too, varies, being sometimes of a clayish or ash-colored hue ; at others, they are composed of dark, tarry, fuliginous matter. Sometimes they seem to be made up almost entirely of water and bile, which, in certain cases, is secreted in immense quantity. The two last mentioned evacuations, it has been supposed, are caused by the action of calomel ; but I have met with them often in cases treated by myself, in which not a grain of calomel or any other mercurial had been employed. The dark and tarry dejections have also been esteemed a certain sign of a favorable termination ; but I have met with it repeatedly in post mortem examinations in the largest quantities—the bowels being literally full of this substance.

The tongue also presents different aspects. Instead of being dry and furred, it is frequently moist—preserving, indeed, an appearance almost natural throughout the case. In truth, those who see much of yellow fever soon learn to distrust the tongue in their prognosis.

Though most of the fatal cases are accompanied with black vomit, the rule is by no means universal ; nor is black vomit necessarily a fatal symptom. I have never passed through an epidemic in which there were not a few cases of recovery after decided black vomit had been thrown up. In one case, it was thrown up for two days, and recovery took place.

Towards the close of the attack, a most distressing hiccough sometimes supervenes. It is exceedingly obstinate, and will yield to nothing ; though not always fatal, it is nevertheless a very dangerous symptom.

The skin, too, presents many and remarkable varieties of color. Sometimes it is pallid, or of a bluish hue—at others of a beautiful pink, diffused more particularly over the chest, throat and face. Its usual color, however, is (towards the close) of a dun yellow. In some cases the yellow hue comes on only after death.

Active hæmorrhages from the nose or bowels sometimes occur on the first or second day. These must not be confounded with the passive hæmorrhages already spoken of. When not too severe, they are rather favorable than otherwise.

Such are the most prominent symptoms which characterize yellow fever during the life of the patient. Let us now follow him to the dissecting rooms, and see if we can ascertain from the dead body the causes of this frightful train of morbid phenomena.

#### POST MORTEM APPEARANCES.

*The Skin.*—Our attention is first called to the state of the skin. A few hours after death, nothing is more common than to find all the lower or



depending parts of the body in a state of congestion—literally black from accumulation of blood. And this is not confined to the external parts; we shall find the same thing at the base of the brain; in the depending portions of the intestines, and particularly of the ileum. We shall also find this congestion more or less in the lower portion of the lungs, and I believe it constitutes, in many cases, what is mistaken for inflammation of the stomach towards the cardiac orifice. As I have already observed, the tissues seem to be partly disorganized by the disease; they have lost, in a great degree, their natural elasticity; the capillary spaces are enlarged, and the blood settles down from the mere effect of gravity.

The yellow hue of the skin, what is it owing to? An obvious answer is, that it is caused by the absorption of bile. But this admits of much question. The color, in the first place, does not so very closely resemble the yellow skin of jaundiced persons—an experienced eye may perceive the difference. In jaundice, the urine is colored by bile; but it is not so in yellow fever. To be sure, jaundice sometimes supervenes in yellow fever; but this is rare, and when such is the case, the urine is tinged. I rather attribute the yellowness to the change of condition which the blood and tissues have undergone. We know, indeed, that a similar appearance may be produced artificially—by a contusion, for example. And, indeed, the color of old contusions resemble the yellow skin of patients in this fever, much more than that produced by jaundice. Again, in cases that die of jaundice, we shall generally detect a complete obstruction in the ductus choledochus, an obvious cause for the absorption of bile. In yellow fever subjects, though I have often made the investigation, I have never discovered any such obstruction.

But let us open the body.

*The Brain.*—This organ is sometimes congested with blood; at other times it contains a little water in the ventricles and under the arachnoid. The pia-mater is sometimes finely injected; the dura-mater is rarely affected, and when so, presents only a few small sanguineous spots on its serous surface. In most cases the brain presents no appreciable lesions whatever. The like may be said of the spinal marrow and sympathetic ganglia.

*The Lungs* are sometimes obviously congested with blood. They do not retract, as they usually do when the sternum is removed. They are also, in parts, much discolored. In one case, examined in 1839, a portion of the left lung, about the size of a dollar, was found in a state of apoplexy—

the blood was extravasated and coagulated. The mucous membrane lining the trachea and bronchia, is also in many cases finely injected, or spotted with blood. In numerous cases—perhaps in a majority—the lungs present no lesion that we can detect.

*The Heart* is very rarely, if ever, affected. The endocardium is sometimes slightly discolored, but I believe this is only met with in subjects that have been some hours dead, and appears to be occasioned by absorption of the coloring matter of the blood left in the cavities. Small blood-spots are also sometimes found on the endocardium, and seem to be analogous to the petechiæ on the skin. We generally find coagula in the heart in this disease, but they contain more coloring matter, and are softer than those we meet with in cases of death from most other diseases.

*The Liver.*—I have never seen any lesion in this organ which could be attributed to the effects of the disease. There is no organ in the body which presents such various appearances as this; at times, being very dark; in other cases, presenting a pale yellow aspect. In the cases examined at the Charity Hospital, it is not unusual to meet with chronic affections of this organ, but as we meet with an equal number of cases at other seasons, it is plain they have nothing to do with yellow fever—either in cause or effect. Indeed, as I have heard it sensibly remarked, there is no organ in the body with which the disease may take so many liberties, without material injury to health as the liver. We frequently find it in conditions which are evidently of long standing, and such as to produce astonishment that the individual could have lived without manifesting his disease, by striking and unequivocal symptoms.

The liver sometimes contains less blood than we usually find in the viscus, and in those cases it is paler and drier than usual. At other times, however, it is engorged with blood, and bleeds freely when cut; but these appearances it is subject to in common with all the organs, and the existence of one or the other appears to depend much upon the condition of the patient at the time of the attack, and the treatment he has undergone. In cases in which the lancet has been used freely, we shall generally find a pale yellow liver.

*The Gall Bladder*, in most cases, contains its usual quantity of bile, which is, to all appearances, healthy. Sometimes it is greatly inspissated; in other cases the bile is more mixed with mucus than usual. I have sometimes found the gall bladder containing only a little glary mucus; these cases are rare. The mucous coat of the organ is sometimes, like other

mucous tissues, injected or spotted with blood. In most cases it is not affected.

*The Spleen* is usually sound. It is sometimes engorged with blood, and in consequence, is enlarged and softer.

*The Pancreas.*—I have never seen this organ diseased in yellow fever.

*The Urinary Bladder* is rarely changed in appearance. Its mucous secretions are sometimes increased in quantity, and in a few cases the mucous membrane is dotted with small points of blood.

*The Kidnies* sometimes contain a great deal of blood. When cut into, we can seldom find any appreciable lesion. In some cases, the mucous membrane of the pelvis and infundibulum is, like the mucous coat of the bladder, minutely spotted with blood.

*The Stomach and Intestines.*—In a great majority of cases, the stomach is finely injected with blood. Not only is the mucous membrane thus discolored, and that, too, in cases examined almost immediately after death, but we find also abrasions of the tissue in pit-like holes and furrows. It is beyond all doubt that this engorgement existed before death. In some cases the whole surface of the stomach is affected; in others, the effusion and injection is confined to the cardiac or pyloric portion. The sub-mucous cellular tissue is also generally injected. The duodenum and a large portion of the other small intestines are frequently found in the same condition.

In some other cases, though these are far more rare, the stomach, duodenum and other intestines present us with an almost entire absence of appreciable lesions. Prying and curious eyes have found here and there a few slight red spots, and have convinced their owners that they detected thickening and softening of the mucous membrane. I confess that I have not been able to see this, and the conviction has been forced upon me that in these cases we could not lay our finger on any lesion which would account for the foregoing phenomena, or the death of the patient. But by these remarks I do not intend to be understood as inferring that these organs have not been diseased. On the contrary, I believe that in all cases of death from yellow fever, the stomach suffers, and suffers most severely. All I contend for is, that we are not entitled, from the facts before us, to say that it has been inflamed—and inflammation, in my creed, is not synonymous with *disease*—but expressive merely of a particular species of morbid phenomena. Those who contend that the stomach must necessarily have been inflamed, reason, not from facts to theory, but from theory to facts.

They are guilty of hypothetical, not inductive reasoning. The stomach has doubtless suffered, since all the organs in the body—every tissue—must, more or less, be altered in its constitution, after undergoing such violent morbid actions. But in the cases we now speak of, the stomach seems to have suffered not a whit more than the other organs.

As the stomach usually contains black vomit, it may be argued that the tissues have relieved themselves, by pouring the contents of their vessels into the cavity of the organ; but if this be true, how comes it, that in other cases, in which we also find the stomach full of black vomit, we meet with a mucous membrane literally engorged with blood?

The large intestines and the lower portion of the small, are not so often found congested as the stomach and duodenum, yet such a condition is by no means rare.

Before leaving the stomach, it may be well to remark that, in some cases, it presents a very singular aspect caused by artificial means. It is difficult to describe. The mucous tissue, when washed, has a sort of marbled appearance, with faint lines running here and there, and intersecting each other. This is owing to the acid solution of sulphate of quinine administered in the latter stages of the disease. That such is the fact, has been proved by immersing a stomach not having this appearance in a solution of quinine.

A remarkable feature in yellow fever is, the frequent occurrence of intussusceptions of the small intestines. These were exceedingly common in autopsies made in 1839. The quantity of intestine invaginated sometimes exceeded a yard.

In certain cases we found Brunner's glands presenting a miliary aspect. Whether this was connected with the disease or not I do not know.

In some cases of a typhoid type, in which there existed before death a low nervous delirium, we found, sometimes ulceration, and at others, hypertrophy and softening of Peyer's glands.

Ulceration sometimes occurs in Brunner's glands, but rarely, and when it does take place we generally have hæmorrhage from them.

The mesenteric glands are sometimes considerably enlarged. This occurs generally in cases in which death occurred after the seventh or eighth day. I have also seen them much enlarged in cases treated on the mercurial plan.

*The Blood.*—This fluid does not present to us any strong evidence of those changes which we might expect after the system undergoing such vio-



lent actions. It has been said that it loses its coagulability. Whether such is not the fact in certain cases, I will not undertake to say, but in a great majority it is certainly not true, for we find coagula in the heart, and blood taken from the larger vessels generally coagulates after a while. It is unquestionably true, however, that it requires a much longer time to coagulate than blood usually does, and that the coagulum is larger and softer. Blood drawn from the arm rarely, if ever, presents a buffy coat; nor have I ever seen it cupped.

*Black Vomit.*—Of this fluid mention will be made under the head of Pathology.

The rest of the body, such as the cellular, fibrous, cartilaginous, osseous tissues, etc., present nothing remarkable. The muscles in many cases are darker and drier than usual.

#### PATHOLOGY.

The nature of this disease must be inferred from the history of its appearance—from its course and symptoms—from some points in its treatment (to be spoken of hereafter,) and from the lesions found after death; for the special cause of it is utterly unknown.

Without troubling ourselves with nosologies or general treatises, it will suffice for our purpose to say that from the earliest times medical writers have in general admitted the existence of two great divisions of fevers, which we may designate by the terms symptomatic and idiopathic. How do these fevers differ from each other? And to which class does yellow fever belong?

A person in good health breaks an arm or leg; receives a pistol shot; is burnt or scalded; in short any mechanical injury may be inflicted on the system, and after the cold stage (into which he is at first thrown) passes over, he is afflicted with high fever. Examining the seat of injury we shall find it to be inflamed. Now, the fever supervening in such cases as these, is consequent, as must be obvious, to a local affection—the phlegmasia—or local affection precedes, and is unquestionably the cause of the fever or general affection.

The means by which the local affection becomes general are the nerves and spinal marrow, the centre of the nervous system; the condition of the nervous substance at the seat of injury is propagated to the spinal marrow; that organ, in truth, takes on the same condition; the nerves going out from it to all parts of the body, are in turn affected—the affection is transmitted to every molecule of the tissues—the relation between the tissues

and the arterial blood is changed, and the local affection becomes general. We have fever supervening on a local inflammation.

In the cases above cited the original cause of all this is known—but it frequently happens that we cannot determine the cause of the original inflammation. Thus we often meet with cases of hepatitis, nephritis, cystitis, etc., which we cannot trace to any mechanical or chemical causes. But we must in such cases reason from analogy, and infer their existence. We know that cold applied to certain parts of the body, whilst the rest remains covered, will produce rheumatism and fever. In some cases pneumonia or inflammation of some other organ is the consequence. Indigestible diet produces gastritis, enteritis, or dysentery. In short, we can trace these connections in so many cases as to enforce the belief that all these fevers arise from the same general cause, viz: from local lesions produced by mechanical and chemical agents. They may also be produced through the influence of the nervous system, particularly from irritation of the spinal marrow as in some cases of acute rheumatism. Certain conditions of the blood appear, also, to give rise to local inflammations which may be followed by fever, as anthrax, for instance.

We have mentioned *chemical* agents as among the remote causes of symptomatic fever. But in all such cases it must be understood that the chemical agent acts *locally*, and thus in the same way as mechanical causes. Thus, boiling water will produce a scald; red-hot iron, a burn; caustic potash and mineral acids destroy the skin; arsenic and corrosive sublimate disorganize the mucous membrane of the stomach, etc. When the chemical agent is absorbed into the system, the effects are complicated, and the general affection assimilates to idiopathic fevers.

The fevers thus produced by local lesions will of course be modified in their symptoms, duration and results, by all those causes which modify disease in general. The condition of the patient—the organs affected—his constitution—the treatment employed, and many other minor circumstances, must of course produce their effects. Of these causes of modification, this is not the place to speak, but in many important particulars it will be found that symptomatic fevers differ from the idiopathic.

In the first place, their duration is by no means circumscribed within the same limits. They may sometimes be cut short, as it were, by judicious treatment, and the patient be convalescent in a few days, or perhaps hours. Even after they are well formed, depletion appears to abridge their duration. In other cases, from peculiarity of constitution, neglect or injudicious treatment, they may be prolonged for months, taking on the chronic

form. Now, in idiopathic fevers we see no such thing. No one ever cut short a typhoid fever, small pox, scarletina, etc., by the use of the lancet. The disease will run its course, and the part of the physician is to watch its progress, interfere when important organs are affected, and restrain the officious interference of others, and the imprudence of the patient himself. To attempt to *cure* the disease, as we may cure a pleurisy or pneumonia, in the commencement, is just pure absurdity.

Again, though not universally, symptomatic fevers are generally accompanied by local pains—pains referred to the organs in which after death we find unequivocal proofs of pre-existing inflammation. Thus, pleurisy is accompanied by pain in the chest; gastritis, by pain in the epigastrium; enteritis, by pain in the abdomen, etc. All this is strikingly in contrast with the general course of idiopathic fevers. There are certain pains common to all of them; pains in the head, loins, and inferior extremities. Small-pox, simple continued fever, yellow fever, etc., may all set in with exactly the same symptoms, namely, high fever, pains in the head, loins, and limbs, so that it is impossible to make a diagnosis simply from the symptoms. We infer the existence of this or that disease from other circumstances, or its nature is developed in its progress.

The blood, drawn by the lancet or otherwise, also presents some peculiar and interesting differences in these two classes of fever. In the symptomatic, we have the buffy coat, and frequently that phenomenon called “cupping.” These phenomena, it is now well known, are owing in these fevers to an increase of fibrine, and though they may not always appear, yet we are assured by Andral\* that such increase of fibrine does always exist. “In the phlegmasia,” says he, “there is an excess of fibrine relatively to the globules, that is to say, the reverse of what takes place in typhus.” (p. 61.) Again: “No matter what may be the state of the system, the representation of an accute phlegmasia involves necessarily, and in every case, the increase of the fibrine of the blood beyond its normal quantity. This law, too, prevails amongst animals as well as amongst men, as I have become well assured from analysing the blood of dogs, horses, neat cattle, and sheep, attacked with various inflammations which had been discovered during life, and after death.” (p. 67.) And again: “The formation of an excess of fibrine in the blood is uniformly the accompaniment of inflammation of a grade high enough to give rise to fever; but this fact acquires a greater importance, both as a diagnostic sign, and as an element of the doctrine which relates to the production of disease, if we reflect that

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\* Essay on the Blood in Disease. American edition.

it never takes place, unless an inflammation exists somewhere. An excess of fibrine in the blood becomes, therefore, a pathognomic sign of such inflammation." (p. 78.) He also says that the increase of fibrine is strictly proportional to the intensity of the inflammation, and a degree of symptomatic fever accompanying it.

Now let us turn to the idiopathic fevers which Andral terms *pyrexia*, in contradistinction of the *plegmasia*. He says: "In my first memoir upon the alterations of the blood, I have proved that the fibrine *never augments* in the *pyrexia*, supposing them divested of all phlegmasial complication; that it often remains in normal quantity, and that sometimes it diminishes to a point at which we do not find it in any other acute disease. I have shown that the pustules of variola, and the dothi-enteric *plaques* of typhoid fever, do not have the power of increasing the cipher of the fibrine; and finally, I have shown that with all the possible proportions of globules, whether they were very abundant, or whether they have become very rare, a *pyrexia* could equally arise with all its varieties of form and gravity." (p. 53.) I must refer to this valuable and interesting work for further particulars; yet I cannot refrain from extracting the following passage which bears powerfully upon the subject in hand.

"At every period of clinical observation, and upon whatever theoretical point of view the observer was placed, it has been recognized that amongst the *pyrexia*, there were some unattended by any grave symptoms, which marched naturally towards a favorable termination; while there were others which, either at their commencement, or during their course, were accompanied by accidents of such a nature, that it seemed as though the forces which rule the organism were either vanquished, or profoundly disordered to such an extent that the extinction of life must be the consequence; and at the same time, it was found that in such cases the blood presented an altogether peculiar appearance; it was observed that, as it became less consistent, it seemed to tend towards a sort of dissolution. Admitted at all periods, but differently explained, according to the prevailing theories, this condition, which may develop itself in any *pyrexia*, and towards which several seem to tend naturally, has been called, turn by turn, putrid, adynamic and typhoid state; it has its greatest development in the typhus fevers, properly so called; it is, in some sort, inherent in them; it is, as it were, their essence. The *pyrexia*, now called typhoid fever, presents it in a slight degree from the invasion, and the grave cases of this disease are its marked representation. It does not ordinarily exist in the eruptive fevers, but it often complicates them, and constitutes one of their dangers. Finally, in addition to the *pyrexia* with well marked characters, which have



a fully determined place in nosological systems, there are others to which no name has been given, which may yet present in a high degree the symptoms to which the ancients attached the idea of the putrid state. This is because there may exist in effect, in all the *pyrexia*, a common alteration of which the blood is the seat, and whose existence constantly coincides with the appearance of those phenomena always the same, attributed by vitalism to *adynamia*, by solidism to relaxation of the fibre, and by humorism to putridity of the humors. This alteration of the blood consists of a diminution of its fibrine; it is consequently an alteration, the inverse of that which betrays in the blood the phlegmasial condition."

But the most remarkable difference in the two classes of fevers exists in the lesions found after death. In symptomatic fever we almost invariably find coagulable lymph effused either into the substance of the organs or on the serous tissues; and in many cases this lymph is partly organized. The mucous tissues, too, in certain instances, as croup, bronchitis, etc., effuse a matter which, if not identical with coagulable lymph, is exceedingly like it. In other cases we have pus, or muco-purulent matter thrown out from these membranes, and almost always meet with a fine *arterial* injection of the mucous, or sub-mucous coat. Now, although I do not deny the occasional occurrence of coagulable lymph and pus in idiopathic fevers, such occurrence is by no means general; in fact, it is the exception, not the rule. In the most malignant, most fatal cases, those which run their course most rapidly to a fatal termination, it is notorious that we find nothing of the kind. Who has ever observed hepatization of the lungs, sphacelus of the stomach, effusions of lymph on the pleuræ, pericardium, peritoneum, etc., in such cases?

We may here digress to say a few words concerning the local inflammations sometimes found in idiopathic fevers. A person may be laboring under a phlegmasia at the time that an attack of idiopathic fever comes on; the inflammation may run a certain course, and its effects be produced, before the idiopathic fever supervenes on the symptomatic. If, as hereafter will be maintained, idiopathic fevers all arise from poisons, this can easily be conceived. Again, the patient may be attacked with a phlegmasia while in a state of convalescence from an idiopathic fever, and death be the consequence. In such cases we may have buffy coat on the blood, or effusions of lymph, etc. And again, the nature of the morbid cause may have its effect. If the poison be mild in character, and not such as to overwhelm the nervous system at once, the reaction of the system may bring

on active local congestions, particularly in robust constitutions; and this, it appears to me, causes the difference between synocha and synochus. The type of each is not produced by the quantity taken, or by the intensity of the poisonous agent, but by a difference of *kind* in the poisons. The like may be said of *synochus*, *typhoid* and *typhus*. It is true that some confusion has crept into pathology from the fact that these terms are often applied to mere conditions—thus, we frequently hear of typhoid cases of small-pox, scarlet fever, etc. This is to be regretted, but cannot well be remedied. A distinction must therefore be made in the application of these terms; for, although persons in weak health, of anæmic habit, or ataxic temperament, or other vice of constitution, will present certain symptoms in any epidemic different from those of the strong and robust; still, independently of these individual varieties, idiopathic fevers have general characteristics marked in their accessions, symptoms, course and termination—and by these we name them. These general characters, preserved through whole multitudes of patients, can only be referred to a peculiarity in the cause producing the disease.

But to return from this digression. We have seen that idiopathic fever rarely presents those morbid lesions in the tissues, so common in the symptomatic. There is still another fact with regard to these lesions to be here mentioned. Symptomatic fevers arise from local lesions; thus, in pleurisy an affection of the pleura precedes the fever; on the other hand the lesions found after death, in idiopathic fever, are the consequences of the disease, and are developed during its course.

Finally, the sudden accession of the fever, the rapid and complete prostration of the powers of life, the tendency to passive hæmorrhages, the appearances of what are termed *crisis*, and the rapid amelioration which follows, together with the effects of remedies, all go to show conclusively, that the idiopathic are diseases essentially different from symptomatic fevers.

The causes of idiopathic fever must therefore be entirely different from those of the other. What are those causes? We know that some of them, as in small-pox, are organic poisons, and it is highly probable, that most, if not all varieties of idiopathic fever are produced by the same class of agents. But we must refer our remarks on this subject to another head.

After these observations, it can scarcely be necessary for me to add, that I regard yellow fever as an idiopathic fever, *sui generis*, and to be caused by a poisonous agent, also *sui generis*, and of an organic nature.

Taking for granted, then, that yellow fever is produced by poison (an assumption to be maintained hereafter,) the question that first presents itself is, how the poison affects the system. Poisons can operate on the body in three ways only. 1st. By attacking and chemically changing the tissues; such is the action of corrosive sublimate, arsenic, mineral acids in a concentrated state, etc. 2d. By acting directly on the nervous system, that is, by affecting the nerves expanded or diffused in the tissues, which affection is transmitted to the central organs. 3d. By being taken into the circulations. Of the first of these modes, it suffices to say, that though the poisons might, thus acting, produce a symptomatic fever, it is obvious from what has already been said, they could not produce an idiopathic fever; moreover, idiopathic fevers do not set in with such lesions. With regard to the second mode, though I shall not here deny in toto the sympathetic action of poisons, still the experiments of Magendie, Delille, Emmert, Vernière, and a host of others, sufficiently prove that they act principally through the medium of the circulation, and render it extremely probable that their specific constitutional effects are produced in that way only. The experiments of Hering and Blake on the rapidity of the sanguine current, also add great force to such a view.

I shall then take for granted that the morbid cause of yellow fever acts upon the system through the medium of the circulation. In what manner does it enter the blood? There are, obviously, but three channels of admission: by the skin, by the intestinal canal, or by the lungs. The skin is protected by the epidermis, an obstacle to absorption, and we may therefore justly conclude that the passage of the poison is in some other direction. If we suppose that the poison is soluble in water, our drinks may be the vehicle of its introduction into the system. If we suppose that it is held in solution in the atmosphere, the lungs are evidently the channels by which it is introduced. It is most probable that the latter supposition is the true one; for the lungs present a vast extent of surface, and absorption and exhalation are continually going on through the air vesicles. Not only is the oxygen of the air taken in, but many other fluids, contained in solution in the atmosphere, are also absorbed; thus, persons sleeping in a newly painted room give from their urine the odor of turpentine; and not only is carbonic acid gas thrown off from the system, by the lungs, but the odor of many substances, such as camphor, onions, etc., taken into the system by other inlets, may be detected in the breath.

The poison having entered the circulation, other questions arise. Does



it primarily effect a change in the blood—and is that change a necessary forerunner to the constitutional symptoms; or is the blood a mere carrier of the poison to the tissues? In the present state of science, these questions cannot be answered.

It is a well known fact that certain poisons remain in the system for a long time without producing any obvious effect. That of hydrophobia is a familiar example. Now we cannot suppose, in such cases, that the morbid agent remains inert. It is thrown by the blood upon the tissues and must produce its effect. But the primary effects may be slight, and thus escape observation. The changes, however, go on—one following another, until the constitutional symptoms are produced. The poison of yellow fever unquestionably acts in this manner, for instances occur in all epidemic years of persons leaving the city in good health and being attacked on board of boats going up the river, or over the lake—or even several days after arriving at the end of the journey. They must therefore have carried the seeds of the disease within them.

We must now examine the *modus operandi* of the poison. Having been taken into the circulation it will wander with the blood to the different tissues. It matters not, for our present purpose, what particular tissue or tissues it may fall upon. It must, whenever it falls, set up a morbid action, and this, we are not obliged to suppose must be one of high intensity—that is, inflammation. The actions of life go on between the molecules of the tissues and of the arterial blood. Any foreign substance in solution with the blood, must produce some perversion of the vital process, be that perversion great or small; the effects will depend upon the chemical nature of the agent. The actions set up may be above or below the normal standard so far as regards intensity—or they may remain of the same intensity. In other words, disease and inflammation are not synonymous terms; and this is particularly observable in yellow fever. The fever subsides on the third or fourth day, the pulse falls to the normal standard, the skin is cool—all evidence of high action is wanting, but the disease goes on, and for several days. The skin becomes yellow, the tongue dry, the urine scanty, the eyes injected, hæmorrhages occur, etc. In short, the patient is precisely in the most dangerous period of the attack. All this is in striking contrast with a case of symptomatic fever, such as pneumonia, pleurisy, etc.

As the poison is carried by the blood throughout the system, it is evident that some portion of it may fall upon the central axis of the nervous system, (the spinal cord) and effect that organ. Now the spinal cord



cannot be affected without every tissue in the body being also affected through the medium of the nerves. Modified in its own actions, it will radiate its affections as from a centre. Hence, in diseases arising from poisonous agents introduced into the circulation, there are *two* sources of the constitutional symptoms—one in the tissues generally, or wherever the poison may be thrown—and the other, in the spinal cord. In this, again, these fevers differ from the symptomatic.

The spinal cord being affected to a certain point, the constitutional symptoms break forth, a rigor is experienced, followed by violent reaction.

But, though this is the usual course of yellow fever, it is by no means universal. We sometimes meet with what are termed congestive cases in which there is no action of a high grade. On the contrary, the system is depressed below the normal state. The pulse is slow, and the hands and feet cold. The term fever applied to such a condition, is evidently a misnomer. We might with as much propriety apply it to a case of collapse in Asiatic cholera, or to a case of poisoning by a large dose of laudanum. The poison in these cases so profoundly alters the nervous substance, that reaction cannot take place. The peculiar disease goes on, however, though fever be absent, as is evidenced by the blood, its want of fibrine, the occurrence of passive hæmorrhages, black vomit, suppression of urine; in short, all the characteristics of the disease.

With respect to the lesions found after death, they are evidently the consequences of the disease; and if any proof were wanting of this, it may be found in the fact that in the worst cases—those of a congestive character—no lesions occur. I regard the lesions in the stomach and intestinal canal, therefore, as I do the yellowness of the skin, the injection of the eyes, the passive hæmorrhages from leech bites, the scant and high-colored urine, etc., not as the causes of the fever or other symptoms, but as effects. They are all but so many different results of the morbid actions to which the system has been subjected; and he who attributes the appalling train of morbid phenomena which characterize this disease to the lesions he finds after death, judges, it seems to me, about as wisely as he who should attribute the conflagration of his dwelling to the ashes which he finds in its place. Nor need I be told that I am admitting the existence of a disease which has no location. I do no such thing. I am as well aware as any one, that disease is no entity—that it is a word expressive merely of perverted actions in the tissues. What I contend for, is, that yellow fever is not gastro-enteritis, or inflammation of any other particular organ—nor proceeds from

such. In short, that black vomit, suppression of urine, passive hæmorrhages, and the rapid and fatal course of the disease, are owing to other causes—to the influence of poison.

But are those congestions which we find in the stomach, intestines and elsewhere, to be considered as the results of inflammatory action? I very much doubt it. We sometimes find them in the lungs, than which no organs are more disposed to the effusion of coagulable lymph. But we never find hepatization of the lungs in yellow fever; I mean, of course, as caused by that disease, not by chronic affections. They seem indeed to be connected with the fever; for the higher the febrile symptoms, the more frequently we find congestions in the different organs. But on the other hand we can witness with our eyes these congestions taking place long after the fever has entirely subsided. We see petechiæ forming, the eyes becoming hourly more injected, old leech bites becoming livid, the gums and tongue swollen and red, etc. Now, in all these cases it is simply passive congestion. There is no action characterizing inflammation, such as increased heat, increase of sensibility, or stronger or quicker pulse, attending the formation of these hyperæmia. If such is the case on the exterior, why should not the same process take place in the interior of the body? There is not one fact to militate against such a supposition.

But at the same time, if these congestions are not evidences of pre-existing inflammations, it must be allowed, that the previous high fever has prepared the tissues for them; since, (as has already been mentioned,) in congestive cases, we rarely find them.

The passive hæmorrhages, so often mentioned, require a few words. They occur, as has already been related, from the bowels, gums, nostrils, etc. The whole system seems to have been profoundly altered. The normal relations between the blood and solids are broken up. The blood arrives in the tissues and passes through, wherever it can find an exit, by the mere mechanical *vis à tergo* action of the heart. It coagulates with extreme difficulty. In many cases every variety of styptics have been used in vain. Even actual cautery is of no avail—the blood oozes forth from beneath the eschar. The only means are mechanical, which suffice when they can be applied.

One of the most striking traits of yellow fever is the occurrence of black vomit. It has been correctly described as resembling coffee grounds in a thin solution of gum arabic, or infusion of flax-seed. But it varies greatly as to color. Sometimes we can see but a few striæ mixed with the flocculent grey matter already spoken of. These striæ are most apt to be found

on the sides of the basin. In an hour or so the fluid ejected from the stomach becomes darker on account of their increase. Sometimes, instead of the coffee grounds appearance, the fluid thrown up approaches in color that of venous blood. In some cases the vomit can be distinguished in nothing from blood in an uncoagulated dissolved state. In short, between decidedly formed black vomit and blood, there are numberless shades. They run into each other by imperceptible degrees, and the distinctions that have been made by some authors in the appearances of the matters ejected from the stomach are altogether artificial.

In the quantity thrown up, there is also great difference in different cases. Some throw up enormous quantities; others die after having ejected but a few striæ.

This fluid has never been subjected, that I know of, to a complete chemical investigation. I think, however, that enough is ascertained concerning it, to satisfy us of its origin and general nature, to wit:

It is composed of solid particles held in suspension by the liquid—since they may be separated by filtration.

It is acid in reaction. Litmus paper is turned red, and turmeric paper, changed by an alkali, is restored to its original color.

A white precipitate is thrown down by nitrate of silver, which is again re-dissolved by ammonia, but not by nitric acid. This indicates that the precipitate is probably a hydro-chlorate. Hydro-chloric acid, it is well known, has been detected in the stomach by almost all experimenters.

A fluid, so like it as to deceive most experienced persons, can be artificially formed by pouring a little hydro-chloric acid upon blood. The addition of a little mucilage will render the resemblance still stronger.

I once, with Dr. Thos. Hunt, of this city, performed the following experiment: A man was brought into the dead house, while we were there. Upon examination, there was no black vomit in his stomach, but a whitish acid-smelling liquid, amounting to about half a pint. Into the stomach, containing this liquid, some blood from the vena cava was poured. At first, we thought the experiment had failed, and we returned to other investigations. Upon examining the fluid, however, after the lapse of ten or fifteen minutes, it was impossible to distinguish it from specimens of black vomit with which we contrasted it.

Now when we take into consideration these facts, and also that, in place of black vomit, it is not unfrequent to find blood in the stomach, and that between blood and black vomit there are numberless shades, we can hardly,

I think, avoid the conclusion, that black vomit is simply a passive hæmorrhage which has taken place from the coats of the stomach. It is absolutely the same in nature with the hæmorrhages from the gums, nostrils, bowels, etc., and different from them in color, merely because it has come in contact with the acid contents of the stomach.

That it is a much more fatal symptom than other hæmorrhages, is easily explicable from the great importance and manifold sympathies of the organ which pours it fourth.

A reason for the frequent occurrence of hæmorrhage in the stomach, may also be found in the structure of the organ—being, at the same time, one of the most porous and lax in its texture, and one of the best supplied with blood in the body.

[*To be continued.*]

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## A Case of Diaphragmatic Hernia.

### THE RESULT OF A PENETRATING WOUND OF THE CHEST.

*Reported by DR. F. L. TANEY.*

Daniel McLeod, aged about twenty-eight years, was admitted into the Charity Hospital on the 1st of April, 1854, suffering with a penetrating wound on the left side, which extended through the seventh rib into the pleural cavity. Both air and blood escaped from the wound; traumatic pleurisy then followed, which lesion, by its consequences, compelled him to remain under treatment until the 1st of June following; in which space of time the external wound had completely cicatrized. He left the Hospital by his own wish, free, apparently, from the serious results of the wound. Yet his subsequent history shows that he did not experience the enjoyment of uninterrupted health, for on several occasions after his departure, he suffered inconveniences, and did, at each time, seek medical aid at the Marine Hospital; and it was on the 1st of March, 1855, that he was brought back to the Charity Hospital, nearly a year after the wound had been made.

His symptoms were much like those of a person suffering from acute gastritis, caused by a mineral poison; but, although the existance of the same signs might be traced, their mode of manifestation was dissimilar. He complained of a peculiar burning in the epigastrium, not at all aggravated by pressure, but attended with restlessness and great anxiety. That



pain, too, was constant, neither increased, nor diminished by the movements of the diaphragm; and respiration was easy and unconstrained. Frequent vomiting tormented him much; drinks and nourishment of all kinds were rejected as soon as swallowed. Accompanying this, a burning thirst compelled him to resort, ever and anon, to cooling draughts; but instead of assuaging, they served to increase, the suffering, and the pain they occasioned in the interval was equivalent to a total privation. Hiccough formed no part in the disorder. The bowels were bound, and all the remedial agents, usually employed in such cases, failed to accomplish the desired end. The urine was scanty, abdomen perfectly flat and seemed not to contain the usual amount of intestines. His intellect was clear to the last.

To these local symptoms were added collapsed features, a pulse small and threadlike, extremities and surface of body cold and damp.

The questions as to the nature of the disease, which naturally arose, were the following: Was the case one of acute gastro-enteritis only? Is it accompanied, or caused by an internal strangulation? And if a strangulation or obstruction, what was its nature? Did it arise from the results of by-gone inflammations—bands of lymph constricting the gut? Might it not be the appendix vermiformis tied around a piece of the intestines? Was it a case of intussusception? But no, nothing of all this. Dr. Wederstrandt, a man quick of thought and sound in judgment, pronounced that it might be an internal strangulation, such as would happen in a diaphragmatic hernia.

The recollection of a case of diaphragmatic hernia, the subject of which was the late Senator Barrow of this State, suggested that this might be a parallel case. Mr. Barrow while in Washington was attacked with the identical symptoms mentioned above, and died after an illness of two days. A post mortem examination revealed an abnormal opening in the left side of the diaphragm through which a portion of the stomach and intestines had protruded, and entered the left pleural cavity, where they remained in a state of strangulation. On further inquiry, a cicatrix was found on his left side, and an old servant related that it had been caused by a penetrating wound of the chest which his master had received about twenty years previously. It appeared that a perforation of the diaphragm had then been made, which had never closed, and which led to the hernia twenty years after.

Daniel McLeod died after seven days' illness. The opening in the diaphragm, through which the viscera had protruded, was situated in the muscular portion, on the left side, perfectly round, three inches in circumfer-

ence; its edge was smooth and thickened; it gave passage to one-half of the stomach—part of the transverse, and descending colon and omentum into the left pleural cavity.

The protruded viscera laid loosely in the chest, and unconfined by any sac; the pressure upon the lung was considerable, in consequence of which it had been flattened against the vertebral column, and kept thus by bands of lymph; the gradual change of the lung explains the easy and tranquil respiration. During life there was dullness on percussion, and absence of respiratory murmur over the left side. This, we may mention, favored also the supposition, of the nature of the case. Nearly all diaphragmatic herniæ recorded in the annals of medical literature have been described as being perfectly devoid of a sac. J. L. Petit, however, mentions a case, of which he says, the patient had suffered for a long time with asthma, and after an inflammation of the abdomen he died.

The autopsy was made, and he found situated on the left side of the chest a tumor, four or five inches high, as large at the base as through its middle. This hernia was possessed of a sac formed by the distension of the diaphragm; that portion of the muscle which formed its base, appeared neither ruptured nor abraded. The peritoneum formed one of its internal coverings.

The cases we have mentioned, like others on record, go to show that wounds of the diaphragm are not apt to close, probably in consequence of the incessant action of that muscle; and that they may prove fatal by giving rise to hernia a long time after their infliction; and this, in a legal point of view, is of vast importance. The knowledge of these facts must be powerful weapons in the hands of justice, helping it to deal the blow on him who disregards its existence, and violates its most sacred laws.

We have sufficient number of examples, to corroborate facts, we have mentioned; in Taylor's Jurisprudence, nearly a similar case to ours, is mentioned. Sir A. Cooper mentions many cases in his work on hernia.

The existence of hernia, nearly always on left side, may appear to be a strange fact; yet, by the consideration of the position of the parts, liable to be at fault, we can not but deem it natural that the accident seldom occurs, only on that side; for unless the rupture in the muscle be excessively large, the liver, by its situation, acts always as an obstacle to the formation of the hernia. The liver has been perforated through the diaphragm, and yet no hernia afterwards is said to have taken place. Laennec relates a case where, after the performance of paracentesis—thoracis on the right side, between the fifth and sixth ribs, it was found, after death, that the instrument had penetrated both the diaphragm and the liver.

Yet hernia on the right side may happen, as we have said, provided the rupture be extensive. Petrequin relates a case, the subject of which had fallen from a height, and had been covered by the falling of a mass of earth—the seventh rib was fractured—the patient died twelve days after the accident. The post mortem examination revealed the rupture of the diaphragm, beginning on the right side, and extending to the left—the whole of the right lobe of the liver, together with a portion of the colon, had protruded in the right pleural cavity.

A circumstance worthy of remark, in the above case, was, the symptoms strongly marked the mode of dying by asthenia,—“the small and weak pulse—afterwards thready and feeble—the depression of the muscular power—the patient, pale and faint,”—all show the great sympathy between the stomach and the centre of circulation—the heart.

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## Acute Phthisis in the Adult.

By DR. THEODORE E. LEUDET, of Paris.

[Continued from last No.]

We have remarked no special lesions of the other genitourinary organs; the urine presented nothing abnormal. The menstrual function in women was constantly suppressed during the course of the disease.

The symptoms of acute phthisis may be summed up in a few words: the disease begins generally by febrile action, more or less violent, accompanied with perversion of the digestive function, dyspnœa and cough; these thoracic phenomena attended sometimes with pains in the side, but more frequently by a sense of constriction at the base of the chest, augment gradually, become accompanied at times with cerebral symptoms, and persist until death.

*Forms of Acute Phthisis.*—We have already adverted to the fact in the preceding chapter, that several varieties of symptoms give a peculiar character to the disease. Acute phthisis we thus divide: 1st. that form which we name acute phthisis, properly so called, in which the symptoms are different from the chronic form; 2d. rapid phthisis, which presents an almost complete assemblage of the ordinary phenomena of pulmonary tuberculization, but runs its course in a much shorter time.

The first species, then, does not differ alone with regard to its duration.



M. Gull is also of this opinion; for, with many other English authors, he calls the chronic form scrofulous phthisis. Sir J. Clark calls it febrile consumption. M. Andral has also observed this form in which the symptoms differ from those which ordinarily characterize the presence of tubercles in the lungs. He expresses himself thus in his *Clinique Medicale*: "There are patients in whom the rapid development of pulmonary tubercles is not even announced by local phenomena; the patient has a slight cough, no expectoration, or, if any, of a purely catarrhal character; remarkable is it that respiration does not appear to be impeded. If the chest be percussed, it is found resonant; if auscultation be resorted to, the respiratory murmur is general and distinct. There however exists continued fever, with abundant night sweats; rapid emaciation takes place, and, the patient reduced in a short time to perfect marasmus, dies before the alteration becomes more manifest." M. Waller, of Prague, in a memoir from which we have often quoted, recognizes several species of phthisis; he makes two grand divisions: granular milliary tubercles, and tubercular infiltration. Milliary phthisis may, according to him, take the following forms: 1st. that of typhus; 2nd. that of acute pulmonary catarrh; 3rd. that of gastric fever; 4th. of intermittant fever; 5th. of delirium tremens;—tubercular infiltration, on the contrary, only resembles pneumonia. This author does not present us with any case in which the symptoms either of gastric or intermittent fever were observed. We have never met with either of these two forms. The greater part of Mr. Waller's work is devoted to the study of the first two varieties he mentions, which we also have found to be the most frequent. We admit in acute phthisis three principal forms: 1st. typhoid form; 2nd. that simulating pulmonary catarrh; and 3rd. the latent form.

The typhoid form is characterized by the general state of the patient; cephalalgia, stupor, sub-delirium, followed later by a more violent form. The face is injected, the skin red and hot, the lips are often covered with sordes, the abdomen becomes tympanitic; respiration is rapid, short and difficult; the arterial pulsations increase in number; subsultus is often met with, and, according to Mr. Waller, the characteristic eruptions of typhoid fever are at times observed. In all the cases observed by us, as well in those of M. M. Gull, Andral and Louis, these eruptions were wanting. The usual characteristics of the rose lenticular spots was not seen. This very incertitude in which we found ourselves, makes us careful in accepting the assertion. The existence of the fact would be a great importance, for then acute phthisis and typhoid fever would be the only two



diseases characterized by this eruption. In this form the local symptoms are the same as in the catarrhal—epistaxis and increased sensibility in the right iliac fossa were wanting. The differential diagnosis of the two diseases will be studied more minutely hereafter. In the catarrhal form, the air passages are the seat of the principal lesion. M. Waller ascribes to it the following characters: The disease begins either by a violent chill, or a cough, more or less severe lasting several days. The respiratory movements are painful, and increased in number; respiration is short, and so embarrassed that the patient remains in the sitting posture, even inclining forward. Expectoration is abundant; in the pain, which is almost always felt, there is nothing characteristic. Percussion gives a normal resonance; respiration is rude—vesicular accompanied by catarrhal ronchi. The face is pale and anxious, the skin hot, the pulse accelerated, the vital forces considerably depressed. The symptoms augment in intensity—in a few days orthopnoea is observed—pallor of the lips and extremities is soon replaced by an asphyxic tinge, and the patient dies at the termination of a few days, with paralysis of the lungs.

We have given almost literally, the description of M. Waller, in as much as it seems to us to present almost exactly the principal characters of this form of acute phthisis. These may be summed up in a word: they are the same as those observed in capillary bronchitis. An important question will be discussed when we analyze the relation which the lesions of the disease bear to the symptoms presented during life. It is this: are the bronchial ronchi to be explained by the existence of some morbid condition of the air passages, or, are tubercles alone the cause of the phenomenon? We may say in advance, that these questions can not be, as yet, solved in a positive manner. Several of our patients, in the outset of the disease, presented but very slight modifications in the voice and respiration; isolated bronchial ronchi, which at first could be scarcely heard, at times rapidly increased in extent, without any appreciable cause. In this form, which we call catarrhal, the local symptoms are not so well marked as the general phenomena, and, as we will see elsewhere, the only error in diagnosis, which it would be possible to make, would be to confound the disease with capillary bronchitis. Expectoration is generally, but not always abundant; the sputa is sometimes streaked with blood; in no case have we found the expectorated matter composed entirely of blood; cough is much more frequent and troublesome than in the preceding form. In his annotations of Laennec's work, Mr. Andral has spoken of this species of acute phthisis; he even mentions a particular kind, without doubt, rare, in which the dysp-

noea, although of the same intensity, is not accompanied with any other pulmonary symptoms but a slight cough, which is neither intense nor frequent. The disease, instead of revealing itself by any well marked or grave symptoms, is manifested at times by phenomena which scarcely attract the attention of the patient, and inspire the physician with a false sense of security; this is the form which Dr. Waller has compared to a gastric fever, but which we more willingly call latent. In one of my observations, the patient, whom we will take as a type of our description, experienced some slight cerebral symptoms, such as pain in the head, and giddiness; but the principal symptoms were anorexia, bad taste in the mouth, nausea and light diarrhoea. This case answers perfectly to the description which Dr. Waller has given to a species which he has named *acute phthisis*, simulating *gastric fever*. The case to which we have had reference was not correctly diagnosed during the life of the patient; the slight fever, with scarcely appreciable pulmonary symptoms, did not lead to the supposition that the lungs were the seat of the disease.

Dr. Waller has spoken also of two other forms of acute phthisis, properly speaking: one which he compares to intermittent fever, the other to *delirium tremens*. This last variety, which we have not had the opportunity of observing, should be studied, principally on account of the complications of the disease; for it is met with generally, if we believe the facts cited by the author, in support of his opinion, when the tubercular matter has been deposited in the membranes of the brain. As to the first variety, which resembles intermittent fever, Dr. Waller has seen but three cases. According to that author, all the forms of a quotidian, and even of a tertian, with hypertrophy of the spleen, have been observed. These periodical febrile symptoms manifested themselves generally during three or four days, but sometimes lasted from eight to fourteen. In short, acute or milinary phthisis generally assumes three forms: the typhoid, catarrhal, and latent; of these three varieties, the typhoid form is by far the most frequent.

[To be Continued.]

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## Communicated.

By J. C. Nott, M. D., of Mobile, Ala.

*Messrs. Editors:*—I take the liberty through your Journal of calling attention to two facts, which I think important, and hope they may be properly investigated by the profession.

CATARACT.—During twenty years residence in Mobile, I have had occasion to operate very often to cataract; and although I have not kept statistics accurately, with a view of publishing, I think I do not exaggerate when I say 4 out of 5 of those operated upon by me have been soft or milky. Has *chance* thrown such a proportion of soft cataracts in my way, or is there any thing in our gulf climate, or other local circumstances, which could produce such a condition of the lens?

When I first commenced operating for cataract, I selected the couching operation, because I did not feel sufficient confidence in my skill to risk extraction. Finding the cataracts so uniformly soft as to be easily broken up and removed by absorption, I have continued this operation. I do not claim any peculiar skill as an oculist, but my success in cataract has been remarkable. My success is attributable to the softness of the cataract, and the simplicity of the operation. If done with a very delicate needle, the operation inflicts very little injury on the eye, causes little inflammation, and requires very moderate skill. Extraction is a hazardous operation, and if the cataracts in Louisiana are similar to those I have met here, the operation by absorption is certainly preferable. I should remark that my cases, with few exceptions, have been in adults, and a large proportion upwards of 50, several 70 years of age.

RHEUMATIC OPHTHALMIA.—Few diseases give more acute pain than this, and all the common modes of treatment are tedious and unsatisfactory. Several years ago, a patient applied to me, suffering intensely with this disease. I cupped, leached, applied soothing poultices, anodyne applications, constitutional remedies, etc., without relief. While in despair, I one night spread some mercurial ointment on a rag, and laid it over the eye—the patient soon felt better, slept well for the first time during a week—next morning was much better, and recovered rapidly. The same thing has occurred to me repeatedly since.

Reflecting on the matter, I recollected a fact much insisted on when I was a student, by Dr. Chapman, of Philadelphia, viz: that mercurial ointment, applied on the face, *invariably produced salivation*. This was stated in his lecture on erysipelas. He cautioned the class against its application to the face in such cases, and stated that he had seen several ladies suffer horribly from salivation produced in this way. I have within the last month touched the gums of a lady by applying it over the eye for violent rheumatic ophthalmia. She has been afflicted with chronic hepatitis for

many years, has taken large quantities of blue mass, but was never salivated until I applied it over the eye.

As mercury, then, is so much more easily absorbed from the face than other parts of the body, I may not be deceived with regard to its therapeutic action in these cases. The fact is a curious one, both to physiologist and pathologist, and may be susceptible of wider application than it has yet received.

MOBILE, ALA., March 16th, 1855.

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## Excerpta.

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ACUTE HYSTERIA IN THE MALE.—By M. Broke Gallway—Surgeon Royal Artillery.—Examples in our own sex of a disease so peculiar to females as to have borrowed its title from an organ distinctive of theirs, must ever be ranked among the curiosities of the literature of medicine. Its connection with—nay, its origin in—derangements of the uterine system is still contended for by the majority of physicians at the present day. Thus, in a lecture delivered by an hospital physician in London, no longer ago than July last, it is laid down that “such cases” (hysteria in men,) “if, strictly speaking, they ever occur, are so rare, that some persons of large experience have never witnessed an example.” Isolated instances of the disorder, however, are to be met with in the writings as well of ancient as of modern authors; and although we may not be expected to subscribe to the dogma of one among the former, that “*hysterica symptomata omnia fere viris cum mulieribus communia sunt*,” its occasional existence in man is too well attested, I think, to warrant the incredulity which many indulge in at the present hour. The following case I believe to be conclusive on the point.

F. H., aged 22, a gunner and driver in the Royal Artillery, admitted into hospital at this station, under my care, August 14 of present year, complaining of looseness of bowels, pains in the limbs, malaise, etc. These symptoms subsided, and he was free from complaint for the following three days, at the expiration of which time he experienced severe uneasiness in the stomach and pain in the head (forehead and occiput.) Did not entirely loose these sensations, though they were benefitted by treatment; and at half past eight P. M., on the third evening after their commencement, he had a “fit,” preceded by pricking and shooting in the thighs and knees. I did not see him in the fit, but it was represented to me as consisting in a staring of the countenance and rolling of the eyes, but of which he himself retained no recollection. In twenty minutes he was again sensible to surrounding objects; but, on coming to himself, he complained of “great agony” in his belly, head and breast, as also of uneasiness in the throat; he described the latter as the seat of a “shooting, pinching pain,” alternating with “heaviness” in the part. In the course of the same night he had a recurrence of the fits. I saw him in the second, and indeed, in several others. Without other warning than a rolling of the eyes, he passed rapidly into a state of insensibility, accompanied with such violent struggling as to require the assistance of several men; his teeth were clenched strongly, but he did not foam at the mouth. Some of these attacks were longer than others, and were always succeeded by a state of insensibility.



Between their recurrence he complained of the same distressing pain in the abdomen and head. He writhed under their severity, and vociferated loudly under pressure of the former. He derived great relief from hot stupes to the abdomen, and the head felt more comfortable under vinegar and water. His general state I treated with opiates and anti-spasmodics. The pupils throughout were lively and unaffected.

On visiting him again on the morning after, I learned that the fits had continued to return, but with longer intervals, and that he had slept a good deal between them; but on coming to himself after one of these, the power of utterance had entirely disappeared. His head was unaffected; and, in answer to queries, he was observed to fashion replies with his mouth, but the power of utterance had gone. He now directed attention to his throat; but instead of complaining of its centre, or of any extended surface, as the subjects of hysteria will do, he carried the point of his finger accurately along the course of the pneumogastric on each side, from its origin, above, in the neck, until its point of submergence into the chest, pushing his finger thereafter, as though he would have followed it in that direction further. A sense of tightness and constriction, or of "pinching," as he described it to me in writing, was the nature of the sensation complained of.

A difficulty in swallowing even liquids was now superadded to his other ailments. Excessive tenderness of abdomen continued to exist, much aggravated upon pressure, with a general uneasiness in the thorax.

In the course of the next twenty-four hours he had frequent returns of the fits, required night and day to be watched, and, when they came on, the assistance of several men was required,

The state of things I have attempted to describe continued for some days, the fits relaxing in severity daily, until the fifth, when he had recovered in all but the ability to speak. In this no corresponding improvement had occurred. He was to all intents a mute, and I watched his condition with interest.

Three days had elapsed since he left his bed. He was allowed to walk about, and divert himself with the other men. He appeared and described himself to me as "well;" "was never better in his life." In the course of the last of these, he suddenly exclaimed, "I can speak!" and did so immediately, as freely and strongly as ever. For above five minutes antecedent to the return of his voice, he experienced a sensation as of "cramp in the throat," accompanied with "difficulty of breathing." Discharged to his duty, August 28.

*Sept. 8th.*—Had been on leave from his barracks during the day; was sober when he came back, at "tattoo," but admitted that he had drunk three pints of porter, for which he was not the worse. Was awakened, at ten p. m., on that night, by a sensation of "pinching and shooting in the knees and thighs," from thence extending to abdomen, and gradually, from last named point, to chest and throat, and finally to head, exactly in the order which I have named. The attack soon amounted to "choking in the throat;" was attended with the same universal implication of the muscular system as before. Insensibility was complete; and, on awaking to consciousness again, his voice had disappeared as before. The duration of the fit was about an hour. On recovering his senses, he declared, and, by his symptoms, evinced the utmost distress in the belly and chest, as also in the head, to each and all of which he constantly pointed with his hand, as the sources of his sufferings.

The second attack was a close repetition of the first. The fits were as severe, the aphonia as complete; a similar time involved from the rise to the fall of the complaint. The voice was as completely in abeyance as before, and its return introduced by a similar sensation in the throat to that described under recovery at first. But there was a feature peculiar to and distinguishing the second attack, which was not a little characteristic of its essence, and which remarkably pointed to the predominance of *nerva* in the play of the morbid processes. It was this, that not a point of his body, from the crown of his head to the sole of his foot, but had become a centre of morbid sensibility. Whithersoever the finger was laid, there, as though the galvanic wire had alighted, an involuntary shrinking occurred—a quick, spasmodic wincing of the part, indicative of positive pain from the touch.

The subject of these remarks was well nourished, and of florid complexion. He represents himself as irascible in disposition, and easily excited. He is wont to shed tears and laugh under insufficient causes; also, when excited, it is not uncommon with him to be inconvenienced by the sensation of a "ball in his throat." During each of his late attacks he passed an inordinate quantity of water; while, in health, he eliminates very little.

The history of the foregoing case is full of interest, physiologically and pathologically. Immediately preceding his first attack, he had been suffering from diarrhoea and gastric disturbance, then very prevalent among the artillery. From the seat of these derangements the irritation seemed to commence and to radiate; and, as far as we are justified in pursuing its invisible track, it appears to me that the terminal branches of the pneumogastric nerve took up and propagated the morbid force, varying it to the origin of that nerve; and that thence, by fresh departures, it occasioned reflex irritation throughout the muscular system at large; in a word, that the irritant was peripheral in its origin, and local, and that the implication of the great centre of intellectual life—the "palace of the mind"—was subordinate in respect of the seat of derangement, and sympathetic in the strict sense of that term;—that the excito-motory system was thence aroused throughout the organism by an influence imparted to the brain along a nerve of special functions, and hence the concurrence of the universal muscular system in the morbid irritative action.

[From the Special Correspondent of the London Lancet]

THE WAR.—*Camp before Sebastopol, December 10, 1854.*—The last letter I wrote to you was from Varna, now some time ago, having been left behind there in charge of the sick. I have now been here a few weeks, but have not had much to say, otherwise I would have written to you before. No doubt you think it strange that the battle of Inkermann afforded no subject for writing, but as the wounded that are operated on are sent down to Scutari, or on board ship immediately, you will see that the result of the cases is unknown to us up here. A few cases of amputation at the hip joint have been performed about five; most, if not all, I believe, did not survive many hours. The wounded Russians gave ample scope for operations. There was a great many, and the nature of the wounds of such a description, that it was rarely a doubtful case came before you; however, I am glad to say, all the poor fellows were treated kindly, and none operated on for the sake of cutting—indeed, there was enough to do without resorting to this unfeeling taste. When a doubtful case came before the surgeons, all points were duly considered, as in hospital practice, though perhaps not so soundly; the patients had always the benefit of the doubt, a few cases doubtless to their own sorrow. Many of the poor fellows were cunning enough to ask for chloroform, which, if advisable, was given to them, and a great many operations were performed under its influence, without, as far as I know, a single accident. The Minie ball generally, as far as I saw, made a very bad wound, and coming in contact with bone, comminuted it a good deal. Some curious cases of providential escapes have taken place. A mounted officer escaped being wounded, by a prayer-book in his holster, which turned the ball. A private of the grenadier guards had one side of his moustache burnt and torn away by a ball, which struck his breast-plate, and glided off to his upper lip. We have had, up to three days ago, some very wet and cold weather, which has done much harm both to the soldiers and their operations. Suddenly a night or two ago the wind changed to the north, and in the morning we found we had a beautiful day and a very hard frost. We were all delighted to see this, though it was cold, as the country was in such a state we could hardly get about at all. I am sorry to say, it lasted only one day; now it is warmer and still fine, though threatening rain, the wind having changed to about south again. The hills in some places are covered with snow, but they do say the heights we occupy are seldom covered at any time. You may imagine the state the country is in when I tell you that it requires twenty-eight horses to drag one of the big guns up, and they had thirty to them a few days ago. High winds seem to be prevalent here and rain, but it is said that about this time there is generally fine weather, and that next month winter begins, which I believe is not very severe.

As you may suppose, sickness is very prevalent, and has increased much lately, doubtless from the weather, diarrhoea and dysentery swelling the list. Fever has its share of victims, and I am sorry to say the fresh regiments that have arrived and the drafts that have just come out to join their various corps, have suffered severely from cholera, which had entirely left until lately, and now seldom or never attacks any one that has been out here for any time. Dysentery and diarrhoea most of the army suffer from—very few escape either one or the other; the latter passes quickly on to the former, which is sometimes very rapid in its course, the patient becoming quite cold, with loss of voice. Nothing seems of any avail; but I think many cases would be saved if they could be removed into houses and kept warm. Fever is not so common as may be expected, but when it occurs it generally is of a low type. Of intermittent fever there has been only a casual case. Rheumatism, which all expected a good deal of, is rarely seen in proportion to the other cases. Bronchitis is prevalent, but does not run on to anything worse. Perhaps a word or two about the duties of the men may account in some degree for so much disease, and the reason we now suffer so much more than the French. In the first place our duty is harder, coming round much oftener than theirs, our army is so small in proportion. The duties are outlying pickets, covering parties, guards and trenches, besides fatigues—most of these are for twenty-four hours at a time in all weathers; and when the men in wet weather come home, we have no change for them, especially lately, as most of their things are worn out, and in the late storm much clothing was lost. Now, most of the tents admit some rain, as the wind was high and beat them in, so a perfectly dry tent is not always to be had. You may imagine these things must cause much sickness, and cannot be avoided. I am glad to say new clothing has arrived, but still there is more at Scutari which cannot be got up for want of means, or rather *will*, of authorities. Our ambulance corps is *hors de combat*, in fact, a failure. Most of the pensioners are either dead or ill; they are estimated as the greatest blackguards in the British army; at Varna they were always drunk, and now, when wanted, are nearly all laid up—their mules and horses have suffered the same fate, so that the wagons are useless for these reasons. Good conveyances are stopped for want of proper drivers. The French are now assisting us to carry our sick to Balaklava. Already we have had recourse to our artillery wagons. Many bad cases we have now, and those which cannot be sent away are not so bad off as they used to be, as we have now light bedsteads, not a foot high, requiring no mattress, issued out to us, though not so many as we require. I believe they are Dr. Smith's invention, and are really very good and serviceable. Lately we have been rather badly off for medical comforts, as they cannot be got up from Balaklava, where there are most things we require; indeed, the roads have prevented the commissariat getting up stores and the forage for horses; each officer is obliged to send for it himself. Still I think it should be seen that the sick do not suffer, and that the regiments should be obliged to assist to get the things up. Each man now is allowed two allowances of rum daily, and while in the trenches three. Potted meats, preserved potatoes, soups. Buckley's cocoa and milk have arrived for the use of the sick—great acquisitions, as you may suppose—with tea, arrowroot, sago and ground rice, if in sufficient quantities. I think no one can in future say the sick are not well provided for, not mentioning the stimulant in the shape of wine. Brandy we are now badly off for—in fact, cannot get it; a very useful article, and its place cannot be filled up by rum in the hospital tent.

Our cause of complaint now is, the ambulance corps not being able to send our sick into the general hospital at Balaklava regularly, without being assisted by the French. If we had an engagement now we should be in a sad fix. Again, not getting our hospital necessities when wanted, on account of not being able to bring them up.

Just as I am writing this the cannons have begun to make a great roar at Sebastopol. This we do not mind, but when we hear the small arms, we think it may be a turn out; but we have got much bolder, and think nothing of it, except when it sounds nearer. We have not had a turn out now for a month; it is astonishing how quick the word runs through the camp, and in a very short time all are under arms and in line. All sleep with their clothes on, and have done so ever



since they landed. The artillery are getting fresh and larger guns up. Report says we are to open fire again with five hundred cannons and seventeen mortars — enough to astonish the natives! I trust it may. At present the fire is very languid, and we only return shot for shot. Something will be done soon — I expect before the end of the month; indeed, the storming will be a frightful affair. If they have the courage to fire all the mines which deserters say are numerous, I think they will do it. They seem determined to hold out as long as possible. You see all particulars about the siege in the papers, so I shall conclude this with wishing you a merry Christmas.

**THE SCOPE AND PRACTICE OF CLINICAL TEACHING** — There is, happily, now no necessity for any attempt to prove the supreme importance of well-directed bedside studies to the student, who, after three years of hospital preambulation — of which, the first often scarcely suffices to render him familiar with the various wards, the visiting hours of the medical officers, and the routine of the numerous departments of a large metropolitan institution — suddenly finds himself in charge of an extensive parochial district, or of a regiment in action, where the issue of a hundred urgent cases depends on his judgment and manual dexterity. We do not undervalue "the School." Physiology must precede pathology; but it is impossible to overrate the practical studies of the ward. Are these sufficiently regulated and "made easy" to the student in our hospitals? Is he systematically trained to practice the first and greatest operation which he will ever be called on to perform — namely, that of making a correct diagnosis! Most assuredly not. Things are better than they used to be in this respect; but there is great room for improvement, more, perhaps, than many at first sight are willing to admit.

Let us not be supposed to assert that we have not men eminently qualified for clinical teaching amongst the surgeons of London: on the contrary, it is our belief that no city can furnish such teachers more readily. But we object to the entire aspect and material of the "Clinical Lecture" as conventionally delivered. The examining bodies have done well to enforce clinical teaching; but attendance upon what is understood as a clinical lecture will not accomplish the end they have in view — viz., the making of a practical surgeon. We therefore propose to indicate briefly here what a clinical lecture ought to be, and subsequently to show how the system might be extended with the greatest advantage to a department of hospital practice where, although it is not now employed, its want is most imperiously felt.

The very term "clinical lecture" is a misnomer. The chief defect in the compositions so designated, arises from their similarity to the systematic lecture of the school. From this they should differ *toto cælo*.

First — A "clinical lecture" should invariably be commenced at the bed-side of the patient, who is the subject of it. Often it may be completed there. Occasionally observations must be made which render this impossible, but the difficulty which presents itself may be easily met by a very little management.

Secondly — It should consist of such an examination of the patient as shall develop, briefly but effectively, the salient points of history, access and progress of attack, and the present symptoms, as bearing upon diagnosis; the treatment and prognosis will easily and naturally follow. The case in question may then be illustrated by similar ones, if they exist, and they doubtless frequently will, for the judicious clinical teacher will select cases, when he can, which admit of such illustration.

Thirdly — The students should be themselves admitted as much as possible to participate in the examination made, and to assist in developing the steps of the operation. The tact of the teacher can alone determine how far this can be successfully accomplished.

Fourthly — The cases examined should include a large proportion of those which most frequently occur, as being those which daily practice will first bring to the young practitioner, and always chiefly engage him in. Also, those of emergency, in which, as "to be forewarned is to be forearmed," a little personal experience so gained, will give him confidence and success. The more rare forms of



disease are not to be neglected, but they should form the exceptional, and not the routine subjects of clinical study.

Fifthly — The examination cannot be too simple or too elementary. The *points* of the case, and not a display of the teacher's acquaintance with what every learned German has speculated thereupon, is that which will stand the student in good stead, whether in the incidents of future practice, or at the portals of admission to the College, it is his early hope to enter. There is no reason why professors should not keep these examinations in mind; it is false delicacy to lose sight of them. They must be passed. Are they more safely met by a preparation at the bed-side, or by poring over the last edition of some popular *vade mecum*? It would be an ill compliment to the examining bodies to say the latter.

Such should be a "clinical lecture," if lecture it must be called. Better, perhaps, a clinical exposition, or a "demonstration of disease at the bedside."

A word about the practical method of dealing with the patient and the students who, as before said, ought invariably to be brought together on the occasion of a "demonstration:" where the class must exceed thirty or forty in number, and the patient can be easily brought into the operating theatre, that is the best arena; unless it be large and extended, like that of Bartholomew's, where the subject of examination and the class would be too far apart. The theatres of Guy's, King's, and University College Hospitals, are better adapted for the purpose. But it would be better still to limit the class to twenty or twenty-four of those students who have passed the first year, as a rule, and to take them to the bed-side. If a quiet corner in the ward, or a small private ward could be devoted to the demonstration, it would be desirable. In either case, a light palisading, in two pieces, formed to enclose an area of a horse-shoe form, and placed upon castors, to facilitate movement, should be temporarily set up, to ensure a clear space of two feet in every direction around the bed. The teacher remains within the area, the pupils without, until the latter are required to enter, one or two at a time, and pass through the space to manipulate, employ the stethoscope, or otherwise observe. In this way a number may be accommodated, while light and freedom of action are observed. A very short time suffices to indicate the salient points of the case, and still shorter to observe them, if the examination be conducted in a methodical manner. There is generally a great deal too much manipulation by students at the bed-side, and that of a kind which is often worse than useless, because the few simple points to be noticed are not distinctly indicated; the efforts of the student exhibited a vague and protracted search, ending in disappointment, instead of a well-directed exploration, which at once seizes and recognizes the physical signs it is desired to demonstrate. Such teaching is nowhere fully carried out. The nearest approach to it may be found, we believe, in the medical wards of University College Hospital.

A subject of still more importance remains — namely, to consider how such a system of teaching could be easily extended far beyond its present limits with the greatest advantage — *London Lancet*.

A RADIO-ULNAR LIGAMENT LATELY DISCOVERED. — M. Denuce in a thesis "on the luxations of the elbow joint" lately published, mentions among other things that near the annular ligament, in which plays the head of the radius, he has, by his dissections, discovered another ligament of about four inches square, inserted, on the one side upon the neck of the radius, and on the other, upon the inferior margin of the lesser sigmoid cavity of the ulna. He calls it *ligamentum quadratum radio-ulnare*; it is supposed to limit the movements of pronation and supination. [*London Lancet*.

ANTAGONISM OF TUBERCLES AND CANCER. — For some time back great efforts have been made to prove that these two diseases can never exist at the same time and in the same patient. To the facts which have already been published in opposition to this doctrine, another may now be added. It was communicated to the Medical Society of Francfort by Dr. Varrentrapp, who found in the same subject both pulmonary tubercles with cavities and cancer of the stomach. — *Schmidt's Jahrb.*, No. 10, 1854.

**CONDIMENTS AND TOILETTE ARTICLES.**—The jury in the Exhibition, or, rather, two distinguished chemists of that Jury, Dr. Hoffman and Mr. De la Rue, ascertained that some of the most delicate perfumes, were made by chemical artifice, and not, as of old, by distilling them from flowers. The perfume of flowers often consists of oils and ethers, which the chemist can compound artificially in his laboratory. Commercial enterprise has availed itself of this fact, and sent to the Exhibition, in the form of essences, perfumes thus prepared. Singularly enough, they are generally derived from substances of intensely disgusting odour. A peculiar fetid oil, termed "fusel oil," is formed in making brandy and whisky. This fusel oil, distilled with sulphuric acid and acetate, of potash, gives the oil of pears. The oil of apples is made from the same fusel oil by distillation with sulphuric acid and bichromate of potash. The oil of pine-apples is obtained from a product of the action of putrid cheese on sugar, or by making a soap with butter, and distilling it with alcohol and sulphuric acid, and is now largely employed in England in the preparation of pine-apple ale. Oil of grapes and oil of cognac, used to impart the flavour of French cognac to British brandy, are little less than fusel oil. The artificial oil of bitter almonds, now so largely employed in perfuming soap and for flavouring confectionery, is prepared by the action of nitric acid on the fetid oil of gas tar. Many a fair forehead is damped with eau de millefleurs, without knowing that its essential ingredient is derived from the drainage of cow-houses.—*Dr. Lyon Playfair on the Results of the Exhibition of '51.*

**TREATMENT OF PLASTIC BRONCHITIS.**—*By T. B. Peacock, M. D.*—In the cases of acute plastic bronchitis which come under the care of the medical practitioner at the commencement of the disease, the aim should be—1st, to relieve the inflammatory symptoms; 2d, to assist the expulsion of the membranes when formed; and 3d, to prevent their further formation. These indications should be accomplished by a judicious employment of antiphlogistic measures, regulated by the urgency of the symptoms and the power of the patient. It would be necessary to cup or apply leeches over the affected part of the chest, to exhibit antimony, and to affect the system mildly with mercury. The expulsion of the membrane might be assisted by the use of emetics, and by inhalations of the vapor of water, or of etheral or other fluids; and the further formation of the membrane would probably be best controlled by mercury. In the later stage, the use of anodynes and tonics would be required.

The use of mercury in conjunction with other antiphlogistic means is strongly recommended by Drs. Gordon and Banks; and Dr. Thierfelder, while he says little benefit results in the acute disease from a purely antiphlogistic course, approves of the employment of depletion in combination with mercurials. He also regards mercurials, the hydrochlorate of ammonia, and the iodide of potassium, as beneficial in the chronic form of the disease. The latter remedy he employed, in the treatment of the case which he has related, in very large doses. Dr. Watson, in alluding to a case of plastic sputum, which was attended with hæmorrhage to an alarming extent, remarks, that though the case seemed at first to call for active interference, and was in consequence energetically treated, on looking back upon it, he regarded the treatment as having been unnecessarily active; and the remark seems applicable to a large proportion of the cases of this description. Though alarming in their first appearance, they often do not seem to require very active measures for their relief. In those cases in which the disease becomes chronic, or recurs at intervals, on any exacerbation of bronchitis symptoms, protection against the weather, and, if possible, residence in a warm climate, should be enjoined.—*Med. Times and Gaz.*

**TREATMENT OF GOUT.**—*By George Budd, M. D.*—The treatment of gout is very plain. Some of our best authorities make it very short indeed, and will tell you, "patience and flannel" will do every thing; but if gout is thus neglected, lithate of soda settles in the smaller joints, and the disposition to gout is encouraged; I would rather advise, when you get the disease in time, with fever, hot skin, full pulse, and gout well-marked, give calomel and colocynth first, with the ordinary

senna draught followed up by carefully administered doses of colchicum. You may give the *acetic extract* gr. i. ad. ad. ii. omni nocte, or the *vinum sem. colchici*. Of the latter you must not give more than a drachm a-day.

It is most important for you to learn how to give colchicum; it is a very powerful remedy, and has accumulated in the system. You should give *vin. sem. colchiti* gtt. xx. ter in die, in the ordinary saline draught; but not with alkaline solutions. The latter may be given in the intervals, but alkalies are said to decompose colchicum, or you may give the colchicum at night and the bicarbonate of potash in the day. Camphor mixture, cinnamon water, etc., may be given with the colchicum, and small doses of the *spts. nitri. dulcis*; this, and the *aq. acet. ammoniæ* will act on the skin, and liver, and kidneys, and thus assist in eliminating the noxious matters of the blood, which assist, if they do not alone assist, in generating this terribly painful disease. We need not give formulæ, as every case will require prescriptions adapted specifically to itself. As a matter of prophylaxis, the physician should advise patients to avoid drinking largely of malt liquors or bad wines; indigestible and unwholesome food also are to be proscribed.

In inflammatory gout you should attend also to the digestive organs, but above everything, malt liquors should be given up; I have known two or three patients make marvellous recoveries by this simple expedient alone. The Bath waters at one time enjoyed a great reputation for curing gout; now, all the world that can do so, go to Weisbaden, where many remarkable cures are made; to these waters I would add gentle tonics, preparations of steel, preparations of bark, the liquor cinchonæ without acid; an overdose of colchicum is known by the patient complaining of nausea; if pushed further, he will have vomiting and diarrhœa, and very remarkable depression of pulse to 50 or even to 40; if there be headache, the dose is too large; we should not give colchicum where there is already diarrhœa or irritable bowels; when given it should be continued till after the gouty inflammation has disappeared, in small and vanishing doses. The limb affected, I need hardly remark, should be wrapped in hot wool, or hot flannel, or, what I have found useful, fomented with the hot alkaline fomentation.—*Med. Cir.*

ON THE VALUE OF THE TREATMENT OF ORCHITIS BY COLLODION.—*By M. Ricord.*  
—Thirty-eight patients have been treated, under M. Ricord, by elastic collodion, according to the rules established by M. Bonnafont. The pain produced by the application of this substance lasted from six minutes to a quarter of an hour. The greater number of patients felt easy from that time, but others experienced a recurrence during the course of the day. One of them complained of most acute suffering. The inflammatory pain diminished in some, and ceased in others, without any sensible influence from the collodion; in a certain number of cases the pain was sensibly augmented. In none of the patients treated in this way had M. Ricord to relate the miraculously rapid cure announced by M. Bonnafont. After twenty-four hours the tumor had not sensibly diminished in volume; it presented, in the majority of cases, the diminution of one-third of an inch after forty-eight hours; then the diminution continued to operate gradually and progressively. The mean duration of the malady in the cases treated by collodion, was seventeen to eighteen days. At the same time, eleven patients, also affected by blenorragic epididymitis, were treated by repose, strict regimen, and topical cold. The inflammatory pain disappeared promptly, and the diameter of the tumor was sensibly diminished at the end of thirty-two hours. The mean duration of the malady was fifteen to eighteen days. A third series of patients was treated by compression by strapping. Here the symptoms disappeared more rapidly than in the preceding plans. The mean duration of treatment was fifteen days. One patient was discharged cured in six days. M. Ricord believes that elastic collodion does not cause such suffering as the ordinary collodion, but that it is a more painful mode of treatment than others. M. Velpeau believes that collodion does not shorten the duration of orchitis; it sometimes diminishes the pain; on other occasions it increases it; it irritates the skin; often excites suppurations, which are tedious and inconvenient. M. Bonnafont has explained his success upon the fact of his having treated his cases at the very outset, a circumstance quite possible in a military hospital.—*Rev. Med. Chir.*



**TREATMENT OF IRRITABLE STOMACH IN PHTHISIS.**—Several very pleasing cases, illustrative of the good effects of hydrocyanic acid and bismuth in the treatment of irritable stomach in phthisis have recently been under observation among the out-patients at the City of London Hospital for Diseases of the Chest. The patients had been long subject to nausea and attacks of vomiting, and were quite unable to retain cod liver oil on the stomach, very often rejecting also ordinary food. The mixture prescribed consisted of three minims of the hydrocyanic acid, and 10 grains of the trisnitrate bismuth made into a draught with mucilage and green-mint-water, and taken thrice daily. By its aid several patients have quite got rid of their troublesome stomach symptoms, and are now taking the oil easily and with benefit. The class of cases best suited to this treatment are those in which the tongue is generally clean, and in which the disease is undue irritability, rather than disordered function.—*Med. Times and Gazette.*

**EXTERNAL EMPLOYMENT OF ACONITINE.**—An interesting case of painful stump is now under the care of Mr. Hilton, in Guy's Hospital. Its subject is a stout, muscular man, whose right hand was removed after an injury some time ago. Ever since the amputation he has suffered most severely from neuralgia of the ulnar nerve, which has quite prevented him from using the stump. The pain was also increased by the least motion. Mr. Hilton ordered the application of an aconitine ointment over the course of the nerve. The ointment consisted in two grains of the alkaloid to an ounce of lard. Very great benefit speedily resulted, and when the man left the hospital, a short time afterwards, the pain had been so completely relieved, that he could use his arm most actively without any uneasy sensation. The cure, however, was not permanent, and a month after his discharge the man was re-admitted. Mr. Hilton did not think it advisable to trust him to use the ointment continuously, and as nothing less seemed to keep the pain in abeyance, it was decided to excise a portion of the ulnar nerve. The operation was performed last Tuesday, a little below the elbow, an inch and a quarter of the nerve trunk being cut out. The case is of value, as showing what the effects of the ointment might be in cases in which the neuralgia was due to a transitory cause. Mr. Hilton has also a man under care suffering from irritable bladder, in whom the same ointment has been had recourse to. By frictions over the loins and sacral region, great relief has been obtained, and the man, who was formerly passing his water every few minutes, can now retain it for an hour at a time.—*Ibid.*

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## Editorial and Miscellaneous.

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"Great men are not always wise, neither do the aged always understand judgment."—*Job: xxxii, 9.*

Nature governs all her concerns, the most trivial as well as the greatest, by immutable laws—and one of her great universal laws is Progress. There can be no stand still. Onward! was the fiat when the revolving planets were launched into space; and onward! was the destiny of man when he was created. When he would hold back from his duty, Nature, that inexorable mistress, drives him, by hunger or by thirst, by ambition or by fancy, by emulation or by his angry passions, to fulfill his destiny.



When we regard them in this light, the orders of "Old Fogies" and "Young Progressives," which have become fully recognized in medical science, cease to appear offensive. As Dr. Pangloss would say: They were necessary, therefore they exist. The former has existed for many ages, but since nature has manoeuvred to make the Doctors cast of their gowns and wigs, lay aside their gold mounted canes, (which took the place of the magician's wand,) shave away their white beards and lay open to the world that their Mystery is not such a great mystery after all, men have begun to breath more freely and to look about, examine, and think for themselves. So the Young Progressives have arisen. All this was brought about that medicine might progress as a beneficial science, and no longer lie dormant as a money-making mystery.

When we consider the mental and physical natures of man, we can see no reason why wisdom should be universally, and exclusively ascribed to the old. Men arrive at their fullest strength, some at a very early, and some at a little later age. They then become apparently fixed for a short while, before they exhibit signs of decay. Most great conquerors, inventors and writers, have performed their greatest works while still young; while all their power have been fresh and vigorous. As in old age the tissues become more and more rigid, so does the mind become fixed in its habits, and the fancy and affections firmly attached to certain objects. Besides this, we think it extremely unreasonable, that he who has been unwise up to the verge of gray hairs, should be accounted wise, to the injury of those younger than he, after he has become fully clothed with them.

But let no one suppose, as some seem to suppose, that the younger members of our profession—or the young progressives—have a disrespect for the old fogies. This disturbance of harmony is indeed the only injury done the profession by the recognition of these distinctions. There is in reality no antagonism between them, except such as is created by the old fogies themselves. Both are, or should be, working for the same object; and we protest that the young progressives are willing to follow, yes, and do follow, with due respect, the footsteps of such of their senior brethren as are worthy of imitation. They are as teachable and have as nice a perception of who can teach them as any other class of men. Yet some old gentlemen are peculiarly jealous of their seniority—and *will* snub the young ones when they venture to form or express an opinion of their own. They seem to think that young gentlemen, by such outrageous conduct, intend to be disrespectful, and to dictate to their betters, and all that sort of thing; and some of them fly into a passion, and run to the printing offices, and make the printers print all sorts of naughty things—for which they ought to be heartily sorry afterwards.

Now, what sight can be more ludicrous than that of an old gentleman, in a rage, stamping his feet, and juvenescently flying around talking naughty, because a young man has ventured to lay his hand on his, the old gentleman's, Hobby-horse, to see whether it stands firm on its legs or no? Instead of being grateful, as a good old gentleman should be, and thanking the young gentleman for having

proved the old hobby-horse, and perhaps saved his dear old bones from a severe fall, if he should chance to rear his hobby-horse a little higher than usual, he gets angry; not, mind you, so much because the hobby-horse is unsound, as because it is *his* hobby-horse. "Sir," says he, "I rode hobby-horses before you were born. I have read Hippocrates on the subject of hobby-horses; and, sir, I have exhausted Galen, Esculapius, Avicenna, and Dioscorides on the subject of hobby-horses. Doctor Francis, Broussais, Brown, Bouilluad, Rasori, Piorry, and I, practiced hobby-horses together under the instructions of the greatest riders of the age. I have sat under Doctor Hosack, and do you pretend to dictate to me! I have long thought you a presumptuous upstart and I shall bring you down a peg or two. You were whipped once while at school; and your grandmother had sore eyes; and once you fell and broke your arm."

As the old gentleman fully expects, some of the by-standers, when they hear his *argument* in favor of his hobby-horse, raise their eyes in admiration at the cotemporary of Hermes, and their hands in holy horror at the young monster. What can the young gentleman do to support himself against a ludicrous old gentleman, in a rage, who has had such teaching and experience? The hobby-horse is broken, and it is useless for him to protest his innocence of evil intent. He is forced to leave, with grief for having given offence to so worthy an old gentleman, mingled with joy that though the hobby-horse may be patched up, and ridden again by him, he will never be able to coax any one else to mount it.

But an infinitely more ludicrous picture than this would this old gentleman present if he should, from a mere nervous fit, a paroxysmal dread of injury to his hobby-horse, fly into the aforesaid rage, disarrange his wig, and get himself into a perspiration at a young gentleman who, he imagines, possibly *may* lay rough hands on his pet. "It is true," says he, "you have not yet touched it, but, sir, you had just as lief do so as not; I see it in your eye; I know the presumption of your heart."

So, thinking that he forsees an attack, and considering the first blow as half the battle, like a skillful old lady, he makes an onset, to confuse the enemy.

Speaking seriously, there is, both in the profession of medicine and law, a nervous jealousy manifested by some of the elder towards their younger brethren, which is, to say the least of it, in very bad taste. Nothing is more unwise and narrow minded. It begets a degree of resentment which is often felt by the senior in his lifetime, but which is invariably visited upon his reputation after his death. The solid foundation of the fame of both physician and lawyer, is that which is fixed in the minds and hearts of the members of his profession. They are the judge and jury who try his worth, and inexorably sentence him to immortality or to oblivion. There is no appeal from their judgment. His memory with the people is like the cloud or the sunbeam of last year, and it soon lies forever with him and them forgotten in the grave. But with his profession it may be cherished as a household word; and, closely allied to science, pass by an unbroken chain throughout all time. Science, too, has its genealogies and nobility; its traditions and its heraldry.

Besides this, what greater compliment can be paid to a man than to be jealous of him.

We do not profess ourselves the special champions of Young Progressives. We seek truth, and shall seize it eagerly, whether it comes from the mouth of babes and sucklings, or from the jaded brain of senility. We proscribe no one because he is young; we follow blindly no man because he is old and has reputation. Young progressives have their rights as well as old fogies. If they be forward, impertinent, or darken counsel by words without knowledge, let them be reduced to proper decorum and taught the truth. If old fogies, be pettish, overbearing, or calumniators, let them too meet with the punishment of him who forgets the courtesies of his profession and the manly dignity of his age.

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UNIVERSITY OF LOUISIANA — *Medical Department.*—The course of lectures in this institution for the year 1854-'55, closed on the 14th of March, ultimo. The class was larger than that of any preceding year, numbering 223 students.

At a commencement, held in the institution on the 19th of March, after a remarkably appropriate and erudite valedictory address, delivered by Professor James Jones, the degree of Doctor in Medicine was conferred on the following gentlemen, fifty-four in number.

Louis Martinez, Robert L. Seal, Samuel H. Brown, Hiram D. Shaw, William Stewart, William H. White, Benj. F. Barry, John M. Rodgers, Joseph F. Moore, Thomas B. Grayson, Ambrose K. Ramsey, Isaac B. Houston, David D. R. Cole, W. M. DeGraffenreidt, William L. Magee, R. W. McLean, James M. Thomson, A. S. Fox, Henry W. Waters, Silas S. Nettles, James M. Calloway, Oliver J. Hood, Josiah B. Hicks, John C. Milner, John L. Worthy, Orlando V. Shurtleff, Robert Wood, Jules Cartier, Angus McKinnon, William R. Horn, Thomas B. Perkinson, Charles Stephenson, Oscar F. Bright, Orren L. Ellis, John G. Allen, James E. Sutton, Lyman J. Rice, William J. Watson, Robert B. Shields, Philip Wyche, Antoine Mouchet, Horatio A. Parsons, Walter Bailey, Cicero C. Meredith, Joseph T. Moreau, Frank L. Taney, Henry C. Snodgrass, Samuel B. Olliphant, John J. Hulse, Henry A. Bowen, John A. Gillmore, William B. Booth, Thomas J. Lockett, Hugh MacDonald Martin.

The degree of Master of Pharmacy was conferred on Edward Aleix.

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SOCIETE ANATOMIQUE DE PARIS.—(*Continued from last number.*)—Diseases of the cartilages, have, of late years, been well studied by some members of this Society; its bulletins, therefore, contain the result of a great deal of research on this point. Mr. Dufour presented several specimens obtained from gouty subjects, and numerous white deposits were found on the articular cartilages. This lesion had, in this case, affected all the joints, and the deposits, composed of urates of ammonia and lime, were found penetrating into the thickness of the cartilages. The



study of lesions, which affect non vascular tissues, has led M. Broca to very curious investigations concerning diseases of the cornea and capsule.

M. Malgaigne once denied the existence of capsular cataracts; not having ever seen any in his practice, he thought himself justified in denying their existence. Mr. Ad. Richard, in a memoir published, even went farther than the able professor, for he endeavored to prove the impossibility of their existence. Two satisfactory cases of *capsular cataract* were this year presented to the society, showing that Messrs. Malgaigne and Richard were wrong, in endeavoring to deny the possibility of their formation.

M. Broca has inquired into the nature of the morbid process which produces opacity of the capsule, and has shown that this morbid phenomenon, which may take place in the cornea, as well as in the capsule, is entirely independent of inflammation, and might be compared to the same morbid process which changes the aspect and structure of articular cartilages. This memoir of M. Broca, contains many interesting physiological deductions. He shows, for instance, that many morbid phenomena take place in the cornea; such as the formation and reparation of solutions of continuity; plastic exudations take place, and are absorbed without the possibility of discovering in that membrane the slightest evidence of blood vessels. Is it an ordinary inflammation? A careful examination of the fact, only admits of a negative answer.

The lesions of the respiratory organs were only represented by a small number of specimens. M. Maingault presented a curious alteration of the lungs, described wrongly by Corrigan as cirrhosis of the liver, which consists in a sort of carnification of the lungs, accompanied with an uniform dilatation of the bronchial tubes. We might also mention numerous abscesses of the lung, supervening upon legating the tongue, one of which opened in the pericardium, and a number of interesting specimens of glanders.

A large number of pathological specimens, relative to diseases of the heart, were presented to the Society. Valvular diseases of all descriptions and hypertrophies of the heart, consequent upon valvular defects or adhesions to the pericardium, have furnished matter to many interesting discussions. M. Barth, in presenting a case of aneurism of the aorta, which had destroyed the thoracic parietes, and projected under the skin, suggested some very practical points in relation to the diagnosis of such tumors. The projection of the tumor towards the external portion of the body, has induced him to believe that the aneurism was *sacciform*, and we know that this kind is generally filled with coagulated blood. The impulse of the blood was of material assistance in diagnosing the size of the opening leading from the artery to the sac. A case of *dissecting aneurism of the aorta*, gave M. Leudet the opportunity of making a very learned report, in which he has brought together all that is known at the present day on that subject.

Ruptures of the aorta are generally preceded by some alterations of the coats of the vessel. M. Bailly presented, however, a specimen of rupture of that artery, without alteration of its coats. A man had jumped from a fourth story window,



and was killed. Upon a post-mortem examination, the sternum was found fractured in two places; the ten inferior ribs, the lumbar vertebræ, and the pelvis, broken; the head of the left humerus was shattered, and, moreover, the aorta was ruptured at its origin; the pericardium open, and the lung itself torn in several directions; the lacerations of the pulmonary tissue existing in the right lung, and unaccompanied by fractures of the ribs of the corresponding side, occurred by a mechanism which M. Gorselin has very well explained in a memoir read before the "Société de Chirurgie de Paris."

Several cases of fibrinous clots of the heart, occurring under the influence of pneumonia, together with some interesting remarks by M. Barth, on the signs which accompany them, will be found recorded in this valuable work. M. Verneuil presented an excessively rare pathological specimen, consisting in the obliteration of one-half of the calibre of that part of the vena cava which extends from the renal veins to the liver. The tube was divided into two longitudinal halves, by cellular adhesions of the internal surface of its parietes.

A great many diseases of the intestinal canal were presented to the Society, but as they contained nothing more than is generally known on this subject, we have passed them unnoticed.

Herniæ are rarely met with presenting identical symptoms, therefore a careful record of the details of each case which presents itself, cannot but be interesting. M. Bidart showed a specimen of inflammation of the perineal sac, with very satisfactory evidence that the strangulation was consecutive to the inflammatory process.

A case of *enterocystocele*, which gave rise to an error in diagnosis, and a case of cystocele, complicated with a hydatid cyst, situated between the bladder and rectum, can also be added to the above mentioned case.

The genito-urinary organs have also furnished their contingent of interesting cases. We will only mention two cases of *tubal pregnancy*, terminating by fatal hemorrhages, and rupture into the peritoneal cavity; a case of tubercular infiltration of the seminiferous ducts; long cylindrical filaments, discharged at repeated intervals from the bladder, and, probably, owing to the occurrence of hemorrhage of the urinary organs.

The large category of accidental productions has been the subject of many controversies. Any one who has had an opportunity of attending the sittings of the "*Société Anatomique de Paris*," has had ample proof of the great distinctions which exist between the different kinds of cancer. We will not here recall the arguments and facts which have established in our mind the correctness of the above opinion; but we will mention the interesting and late researches of M. Foucher, on the pathology of *cysts of the popliteal region*; a case of epithelial cancer of the lip, presented by M. Laboulbène, which gave M. Broca the opportunity of satisfactorily demonstrating the character and progress of the ganglionic engorgement which generally followed epithelial cancers.

The work which we have thus briefly noticed, contains a mass of interesting observations, and the Society of which it is the organ, still stands as the first school of pathological anatomy in the world.

THE WOMEN'S HOSPITAL IN NEW YORK.—We are gratified to learn that a noble institution with the above title has recently been established under the direction of some of the most wealthy and influential ladies in the city of New York. The following *Officers* were elected :

MRS. DAVID CODWISE,	<i>First Directress.</i>
MRS. W. B. ARTON,	<i>Second Directress.</i>
MRS. DR. VALENTINE MOTT,	<i>Third Directress.</i>
MRS. JACOB LEROY,	<i>Treasurer.</i>
MRS. T. C. DOREMUS,	<i>Ass't. Treasurer.</i>
MRS. DR. HORACE WEBSTER,	<i>Secretary.</i>

There is published a long list of Managers.

The following distinguished names constitute the Medical Board :

J. MARION SIMS, M. D.,	} <i>Consulting Surgeons.</i>
ALEX. H. STEEVENS, M. D.,	
VALENTINE MOTT, M. D.,	
EDW. DELAFIELD, M. D.,	} <i>Consulting Physicians.</i>
JOHN W. FRANCIS, M. D.,	

Under such favorable auspices, this noble Institution can hardly fail to succeed, and form another glorious monument to the beneficence of the medical profession. It is organized for the relief of *poor* women, who are laboring under the saddest of human afflictions. We are pleased to see that a Southern physician is at the head of the medical corps, and hope an ample field will now be presented for the display of his eminent abilities.

A new hemostatic has lately been discovered by M. Hannon, of Belgium, which, if the author's account be true, will be of great service in medicine. It is composed as follows :

Benzoic acid,.....	1 part.
Sulphate of alumina and potassa,.....	3 “
Ergotine,.....	3 “
Water,.....	25 “

These ingredients are boiled together for thirty minutes ; the water is then allowed to evaporate until a viscid mass is the result. This is for external use.

In hemophthisis, Dr. Hannon uses pills composed of—

Benzoic acid,.....	℞i.
Sulphate of alumina and potassa,.....	} aa℞iii.
Ergotine, .....	

to be made into sixteen pills, which are to be given, one every two hours.

#### CHARITY HOSPITAL REPORT—Month of March, 1855:

Admitted, - - - - -	766
Discharged, - - - - -	653
Died, - - - - -	106
Births, 7—3 males, and 4 females.	

NEW ORLEANS  
MEDICAL NEWS AND HOSPITAL GAZETTE.

VOL. II.

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NO. III.

Report read to the Academy of Sciences, New Orleans,

IN DEFENCE AND EXPLANATION OF THE

REPORT OF THE LATE SANITARY COMMISSION TO THE CITY COUNCILS,

By E. H. BARTON, M. D., its late Chairman.

*"Dies errorem delet, veritatemque illustrat."*

It was not to be expected that the late Sanitary Commission, in the fulfillment of the important trust confided to it by the Public Councils of the City, would, in the investigation and disposal of the various delicate and long disputed questions before the Medical and Scientific public, so finally put them all beyond dispute, as to leave no ground for dissent. The subjects involved in the discussion have long engaged the profession and the public in the mazes of controversy. It did not flatter itself with the expectation that all these were to be settled at once. But it anticipated from the courtesy of the Profession, in view, no less of the magnitude than of the complicated character of the points involved, (being strictly those of a scientific character,) that the discussion of their merits, their value and the new principles evolved, would be temperate, courteous and strictly confined to the subjects at issue. In this, it has the honor to acknowledge, it has not been disappointed, and that, with one single exception, the Report has met, throughout the whole Union, with a kindness of reception unsurpassed in literary annals. If here there has been any departure from the unanimity which has characterized its reception elsewhere, it is upon points where greater detail and illustration were deemed necessary in the Report itself, and we gladly avail ourselves of the opportunity, to make farther explanation; to fortify points not deemed sufficiently explicit, and to prove, by analogy, what is insusceptible of any other testimony. In pursuance of this intention we shall proceed to examine a paper addressed to the

Legislature by our friend Dr. MCFARLANE—professedly on “Quarantine.”

In analyzing this criticism on our Report, (for the greater part of it is no less,) we willingly confess, that the Doctor occupies a very different position from that alluded to above. He is a gentleman of education, and we cheerfully admit he has fine acquirements, and gifted with a most exuberant fancy, and although endowed with some extraordinarily queer notions, has that characteristic of genius, which makes the worse appear the better reason. In the examination of this remarkable paper, it becomes our duty to say, that he is in the constant habit of supplying a most defective memory, with a most fertile imagination; fancy supplies the place of facts, and the sober and unsuspecting reader is fairly fascinated and carried away by the exuberance of its creation. But neither science nor truth requires such lofty flights, and we shall find it proper to clip the wings that soar beyond their sober dictates, and test their value by the humbler and rather regular standard of reality. The only trouble this examination has cost us, has been to separate fact from fancy—to distinguish the true from the assumed, and to show that assertion is not proof. Archimedes said that “if he had a place to stand on he could move the world”—so with this gentleman—admit his data and his positions are proved. Now, this task of separation has not been an easy one, simply because, with his pruriency of imagination, he has winged his flight over periods where there exists a great deficiency of records. To be sure, it was much easier to deny the verity of his assertions at once—to meet them by assertions in turn, and call upon him for proof, which we knew did not exist. This might answer very well in a court of justice—but in a discussion before the public, would be little heeded.

This gentleman has informed us that he “is the oldest member of the Profession here.” Now age has its attributes as well as its privileges. From age should spring wisdom—the fruit of long and enlightened observation. But one of the sorest attributes of age, is a defective memory and inappreciation of recent discoveries in science. If then, in the course of these observations, I shall have to point out a very remarkable failing here, he has furnished a ready excuse in the patriarchal age he boasts of. But age cannot sanction errors, and time, which writes its furrows on the brow, does not always purify the mental vision. Age and opportunities of knowledge are legitimately to be boasted of, when the evidence of their right use can be pointed out, through successful industry, in exploring the mysteries of our science and in trophies which have benefitted mankind.



These—these are the true triumphs of wisdom. They are putting to their proper use, the splendid education spoken of, and the fine talents we know he possesses. We shall not follow the example he has set us, but leave our cotemporaries to say, if the course we have pursued, in investigating the *causes* of phenomena whose effects have been so disastrous, as to enable us to understand and *prevent* them, is not making a better use of education and the humble talents we possess, and is not being more useful to mankind, and leaving more enduring monuments behind us, than all the corruscations of genius and all the brilliancy of a meteoric blaze.

It will become necessary, in defending our Report, to expose these errors of fact as well as errors of reasoning. If the gentleman has departed from “the established doctrines of the medical profession and of the whole civilized world,” as he boastingly acknowledges, and wantonly sacrificed them for those whose tendency is to keep this country in a *statu quo*, forbidding all hope of amendment hereafter; we, nevertheless, give him credit for good intentions, because we have long known intimately the source whence they emanate. But the consequences resulting, are no less to be deeply deplored, for, if carried out, *as they have been practically carried out here for many years*, they are fraught with vast injury to the city, which we are equally sure he does not see, as they are accompanied with no recommendation for alteration or amendment—thus leaving us nothing but lamentations for the past, and without hope for the future! It was feelings very different from these which gave birth to the Sanitary Commission. This city, with no equal on earth for commercial purposes, has been staggering and struggling under a wretched reputation for insalubrity for many years, which, if it could be removed, as we are very sure it can, her unequalled resources would be at once developed; it was with the full conviction that there was something radically wrong in the constant succession of devastating epidemics—notwithstanding all the “Buncombe” writings and assurances to the contrary, which first gave birth to this investigation into its causes. It would then have been false to their honorable appointment, false to their noble profession, and above all, false to the community which had looked to them for sound advice, and an intelligence, corresponding in some degree, to the enlightened advancement of the age we live in, did we not repudiate all such heterodox notions—*notions not having their basis in common experience*, are unsupported, we firmly believe, by all we know of man’s nature.

The course pursued by our city authorities, for a great many years, has

been empirical in the extreme—never to acknowledge that there was any need for the cauterizing hand of surgery or science, in the face of an annual mortality exceeding that of the most celebrated battles, and in a ratio more than double that of any large city in America. This disastrous course must be mainly attributed to those who, assuming the credit of having examined the subject, have been instrumental in forming a public opinion, whose effects we all see around us. These have asserted that New Orleans is “one of the healthiest cities in the Union,” which was proved by an array of testimonies from various travelers as far back as a century ago! and farther corroborated by deriving the average age at death by selecting it from epitaphs on tombstones! these *irrefragible proofs* have been constantly dinned into the public ear, until finally, it has become a kind of moral treason to admit that people die here at all! and all who attempt to stem this torrent of lies and toadyism, are held up as enemies of the city—as attempting to “write down” New Orleans. And even if one dares to tell the truth in a public document,\* we are accused of saying things “least flattering to New Orleans.” And, really, one must have some moral courage, in the face of a public opinion thus artificially formed, to dare to tell the unvarnished truth. And what is that truth? a mortality of near six per cent., for near half a century, instead of their “unparalleled salubrity!! and an occurrence of ten epidemics of Yellow Fever and Cholera in eight years, with a mortality of 77,338 during that time, or, near half the present population of the city—officially published! To show that this is but the necessary and inevitable result of the circumstances around us—these very precise conditions have existed and produced these very results under analogous climates and conditions, wherever they have existed, and that in proportion to the cause so have been the effect, in the most unvarying manner! When pinned to the wall to account for this frightful mortality occurring year after year, the impudent and ungrateful reply is—“it only consists of immigrants!” immigrants who have made this city what it is, and consists now of no less than two-thirds of the white population. Is it not time that this audacious and miserable system of public imposition should cease, when the reputation of the country is ruined, the hand of improvement palsied and we are losing some of the best of our citizens by emigration? Is it not time that this veil of concealment and falsehood should be withdrawn—and that we should look the facts steadily in the face? Is it not time that the truth should be told

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NOTE—Board of Health Report, 1849.

in all its naked deformity—if we ever intend making an attempt at amelioration or change? Indeed the sanitary condition of New Orleans is not, at this day *a matter of opinion*, with the mortuary returns and the census of the population before us, for half a century or more; it has, unfortunately for us, passed from opinion to *fact*, about which there is no room for dispute by any honest inquirer. The data are not contested and are incontestible. With the variation of these causes so has varied the disease. Can proof be stronger? It would really seem from all the facts, the testimony and the reasoning, that all well constituted minds would come to the same conclusion, where the weakness—where the imperfection of the argument and conclusion is, I confess, I am utterly unable to see; and it is certainly a poor compliment to the intelligence of this people to attempt to humbug them in this way. The people of New Orleans in matters to which they have directed their special attention, have no superiors in shrewdness and sagacity. Upon the subject of the public health, they, like most others, will readily submit to be flattered. The time has come, however, “to speak the truth, the whole truth and nothing but the truth.” No city on earth, notwithstanding its great commercial advantages, can long sustain such drains as we have undergone. It must cease, or there is no help for New Orleans. There has been no attempt at denial of our facts and we are very sure our conclusions are irrefutable.

In relation to the present paper—data are assumed—hypothetical views set forth, and untenable objections to our positions, which I now proceed to examine.

The following quotation embraces these unique views and objections, and I give them in extenso in order to do their author full justice.

[It was here intended to give three-fourths of the entire paper of Dr. McFarlane, to comply with the last remark—but as, wherever objection is made, the objectionable part is quoted, it was deemed superfluous.]

Let us test these averments by the rule he has himself laid down, viz: “*That medical philosophy in order to be useful must be accurate.*” “During 1825 there were not twenty days in which it did not rain violently throughout the whole year!” This, of course, is all *ad captandum*, as there is *no record of rain* falling in New Orleans at that period, and it is just as easy to say it fell every three days; and be it remembered, that all this is from memory, a memory extending back beyond the average age of life here! But there is a mode of arriving at the *probabilities* upon this subject, which although it has cost me some labor, is not without its value, in various

relations to this subject. Failing in records, which I am convinced, after much research, do not exist, I have accurately calculated every record of rain that I have been able to ascertain has been made in the delta of the Mississippi for the last forty years. From these calculations I have learned three valuable facts:

1st. That a very rainy season in one part of the delta is very apt to exist over the whole.\* 2d. That an unusually rainy and sickly season are commonly concomitant; and 3d. that less rain falls in New Orleans than any part of the State, (by near 10.48 per cent.) so far as any records have been made, and these records have extended from the low lands of Plaquemines to the high grounds of Washita! The first is not at all improbable, because the direction of the rain-bearing winds all proceed from the same quarters—that is from the S. E. to S. W., (the Gulf of Mexico and submerged delta.) Of the second, I have before spoken in the Report, and the result of this examination still farther corroborates it. Well then, I have the record of the year 1825 and the following years, made by myself, in this State near the Mississippi river, between 30 and 40 miles farther north than New Orleans, and the record of that very year shows *fourteen inches less of precipitation occurred than the average of 13 years*, and the average of the three following years were almost equally small—they were all comparatively healthy years in New Orleans, as we see by the Cemetery returns, and the strong presumption is were comparatively dry years. The year following this series (that of 1829) was one of the greatest precipitation in that record, it was a year of a severe epidemic in New Orleans, and by a like probability, a very wet year. A large amount of rain fell in the winter and spring months, in the position referred to, (in 1825) and it is probable the same occurred here—nor does it require a great deal of rain, on the much used unpaved soil of the streets of New Orleans to produce deep muddy holes.

Nor is New Orleans actually warmer now than it was 30 to 40 years ago, notwithstanding “the slate roofs and brick houses and pavements.” On the contrary, and on this subject I can speak with the record before me—it is *actually cooler*, from 2 to 3 degrees on the annual average, probably arising from the more extensive clearing and draining in the neighborhood, admit-

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NOTE.—The great value of this information to the commercial as well as agricultural interests of the State, will be readily understood by intelligent men. I have not been called on, from the course of my argument, to extend my researches into neighboring States. But its direct bearing upon the productions of our cotton bearing States, as well as others, is obvious enough, and is eminently entitled to the regularity of National record, to be published monthly, from every parish and county in the South; and it might be made the duty of the Principals of the Public Schools to do so, from instruments furnished by the State.



ting a freer ventilation, and the extension of these very brick houses complained of, which are much cooler than wooden ones.

So, also, yellow fever has been found remarkably fatal with "rag pickers, scavengers, grave-diggers and those who dig in the streets," unless when they consist of acclimated subjects.

Again, to show that yellow fever is neither more frequent nor virulent now than formerly, I have constructed the following table, embracing all the epidemics since 1816—when the ravages of the disease began to be more definitely recorded, and divided them into four periods, viz :

	Average mortality per 1000.	Average du- ration of ep- idemic influ- ence.
The first embracing the short period of 6 years, containing the four epi- demics of 1817, '19, '20 and '22.....	20 30-100	77½ days.
2d. Embracing next 11 years, cont'g the three epidemics of 1829, '32 and '33.	14 62-100	40 "
3d. Extend'g to '42—9 years, includ'g the three epidemics of '37, '39 and '41.	12 25-100	56⅔ "
4th. Extending to the present period—12 years, and including the five epi- demics of 1847, '48, '49, '53 and '54, .....	20 62-100	55 "

But let us proceed farther in testing the *accuracy* of the averment—“that there did not occur a case of yellow fever in New Orleans for six years afterwards”—that is, after and including 1825. The published returns of the Charity Hospital alone exhibits a record of 1344 cases during that very period! and that house usually furnishes from one-fourth to one-third of the mortality of the city. Comment is unnecessary.

But, let us proceed—the term “virginal soil” was not used by the Reporter, “*original soil*” was. The soil, as deposited by the river on its *banks*, is known to contain very little organic matter, and the simple disturbance of that soil alone, it is not believed, or *ever stated*, would produce the influences ascribed to the “terrene.” What is specially denominated and comprehended under this term was distinctly stated to be the rich alluvion of the country—the marsh mud—the detritus and remains of vegetable and animal life, and to be equivalent to putrifiable substances of all kinds, the filth of kitchens, stables, vacheries, privies and every species of filth and offal—the relics of civilized life—from whence proceeds the bad air produced by this disturbance and decomposition. These are found mostly in our back yards, in the gutters, streets, open lots, and are especially conspicuous where our pavements are disturbed, (for the pebble-stone pavement is eminently objectionable in being the *best filterers and retainers* of putrescent organic matter.) The “disturbance” of these is always very offensive in hot humid weather, (the meteorological condition) and injurious to health. So the detritus and filth of our canals and basins,

when dug out or cleansed, are composed, mainly, of these same materials, also the deep cuttings and excavations for our railroads, the *first cultivation* of the soil for agricultural purposes; all are followed by sickness *when the meteorological condition is present* and of sufficient duration. These results are believed to be uniform, the proofs are positive, the facts are not denied. They are evaded by a side issue, which will be examined presently. It was never said, meant or pretended, that cutting through a *sand or clay bank* would produce these effects. Now, *precisely analogous to this* was the composition of the "mud of Camp and Magazine streets, and those *impassable* gulfs of 6 or 8 feet deep, in which hundreds of horses perished in 1825"! These are not offensive to smell, have not much organic matter in them, and are but little injurious to health, except from the humidity they promote, in an unfavorable season. The streets of New Orleans, from Levee to Rampart streets, were composed, originally, mostly of river mud, and afterwards covered with our wretched pavement, and it has never been asserted any where by us, that *that mud*, however "disturbed," would produce disease. But take up any of our ill constructed pavements in the moist summer season, and particularly of the gutters, wherein is deposited and retained most of the filth, and the nose, even at a respectful distance, will detect the difference between the materials filtered beneath them, and common mud. It was for this reason the Sanitary Commission advised the construction of pavements that would neither admit absorption or exhalation. What was asserted is expressed above, and defiance of any exception to the rule, as laid down, is offered. Since the publication of that Report, public attention has been called to the subject — numerous instances have come to light, strongly corroborative of that important truth, and none to oppose it.

The organic poisonous matter, then, is satisfactorily accounted for, without the necessity of looking for it in the "argillaceous deposit," and thus furnishes "one of the blades of the shears of fate."

But it is said — "there being a declivity of 10 or 12 feet from the elevation of the levee to the swamp, one deluging rain is at any time enough to remove all superincumbent filth, and convey it to the swamp in the rear of the city." The folly of this is made apparent, when it is known that *no surface washing* will remove that which is *beneath the surface*, and after a few hours of the "most deluging rain," filthy bubbles of the most corrupting materials, may be seen, during every sickly season (that is when the meteorological condition is present) arising from the subsoil and imper-

fect pavements, where some little water is left. But, again, I wish my friend to be brought down from his lofty imaginings; this beautiful inclined plain of "10 or 12 feet from the *elevation of the levee to the swamp*," turns out to be, that from Levee street (not the top of the levee) to Rampart street, is about six feet, and the balance about two, in as many miles. But in the whole of this course, subject to every kind of obstructions and retardations, so that a rain, say a sudden one of one inch in vertical depth falling in an hour, (a very unusual occurrence) is positively hours in reaching the swamp, and all, who reflect for a moment, are aware of this fact, for in the central portions of the city, where the greatest declivity or inclination occurs, and where the pavements are most extensive, there exists, in fact, comparatively but few obstructions, except bridges, and we all know that it is often hours before we can pass the streets, (after such a rain as above) which then are converted into almost impassable canals and miniature rivers. How much more so is it beyond Rampart street, where this "inclined plain" has more than four times the length, with about one-third of the declivity, and hence the necessity of deeper gutters to carry off the water and filth, onward to the swamp.

"The results of investigations made in this and corresponding regions make it manifest that wherever *heat, filth, moisture, decomposition, exhalation and malaria are combined in sufficient concentration to produce disease*, there yellow fever cannot exist" !!! The italics are his own. All this is truly "the antipodes of the doctrines of the Sanitary Commission," as "IT IS OF ALL THE WORLD." The capitals are mine. The result of investigation!!! why, it is exactly the reverse. The records of the dead—the records of the Profession—the history of every sickly place and country—the experience of all mankind, all accord in *an unanimity, no where surpassed*, that filth (in the enlarged sense) produces disease every where, and particularly in a warm and humid climate and season. So intimately is cleanliness associated with our ideas of health, that it has become one of the strongest instincts implanted in our nature, corroborated by divine revelation, and participated in, as a strong conservative power, by the lower animals, even at the earliest age, and man estimates and cherishes this, just in proportion to his elevation in the rank of civilization and intelligence. The causes producing it are known to be at war with his being—they consist, mainly, of effete worn out matter, of organic materials, passing from one stage of life where, in the order of Providence, they have performed the task assigned them, and fulfilled the

circle of all created things, and are no longer fitted to perform this duty a second time, until their allotted round is past. The Report is most conclusive upon this subject—the facts upon which it is based are *undenied* and *undeniable*. It is there conclusively demonstrated, that filth does produce fever, and with the *meteorological adjunct*, YELLOW FEVER; that in the parts of cities where these are most concentrated, are its peculiar haunts; that these effects are in *pretty precise proportion*, both in numbers and malignity, to the predominance of the aggravated causation—and the very places, spots and houses have been designated, and not satisfied merely with a reference to other distant cities, places and periods, they are pointed out *here, under our very noses and eyes*—the *witnesses are all before us*—the *testimony is direct*—the *facts irrefragable*—they are undeniable; but now, forsooth, to gratify a whim, our friend has exhumed a most fanciful hypothesis from its sleep of centuries, where it never was exalted to any higher dignity than “the on dits of travellers.” But, seriatim, let us see upon what grounds it is now hazarded. Let us examine that “demonstration which is as clear as any proposition in Euclid.”

The first proposition is, that “where malaria is sufficiently concentrated to produce disease, *there yellow fever cannot exist.*”

This has been most thoroughly answered in the preceding paragraph—not occurring where there could be any room for doubt—but before us all, where the real difficulty consists in avoiding to see it; where exists the festering sores of the city’s filth is found the city’s mortality; where year after year are offered up the catacombs of those valuable laborers, who constitute our real wealth, to the insatiate archer, and in lines of lurid light illumine the dark holes and sinks where the innocent victims of erroneous opinions and erroneous action, or want of action, could read a lesson to make humanity shudder, calling aloud for the cauterizing hand of reform, ruining the reputation of the city and blighting her commercial prosperity. But the subject is not left here; it is shown, by the most unexceptionable authorities, living and dead, with a like exactitude of time, place and circumstances—nay, by testimony only limited by historic records, and confirmed by all reasoning from effects to causes, and vice versa—that these effects exist *wherever* these causes do, and that in proportion as they are removed, or gotten rid off, or cease, so subsides or ends the effects. Let us examine the *argument* furnished for its support, in place of and in substitute for *facts*, which are directly opposed to it.

“Yellow fever is different from bilious or miasmatic fever; this, in its



progress and phenomena, is accompanied with visceral engorgement and frequent returns, which is not the case with yellow fever, and that, *therefore*, 'whenever miasmatic fevers exist, yellow fever never occurs.''' The most casual examination of any cemetery return in this city, and particularly that of 1853 and accompanying map, will most fully answer this statement. And what do these silent, but most unanswerable records show? They show, and the demonstration is before our eyes, that in this very city, during the worst seasons and the hottest and moistest months, the filthiest localities are reeking with yellow fever, while the balance of the class (for as I class them all together as proceeding from the same cause, but in a minor degree) of miasmatic fevers—as intermittent, remittent, bilious, congestive and pernicious fevers, prevail at the same time and place, as well as at other periods. The following table, derived directly from our cemetery returns, presents the simultaneous prevalence here of both, in separate columns. A very cursory examination of this table will fully satisfy the honest inquiry after truth, that the same fevers do occur in the same city and locality; the susceptibility of the subject, and the filth, etc., of their habitations constituting the only difference between them.

RELATIVE FREQUENCY AND COTEMPORANEOUS OCCURRENCE OF YELLOW FEVER AND OTHER MIASMATIC FEVERS.  
DERIVED FROM THE OFFICIAL CEMETERY RETURNS.

Year	Disease.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Total.
1853.	Yellow fever.....	1				2	31	1521	5133	982	147	28	4	7,849
	{ Other miasm'c forms, as remittent, inter- mit't bilious, conges- per's, typhoid fevers. }	40	23	20	36	39	55	94	90	89	31	18	25	560
1852.	Miasmatic fevers.....	48	64	63	49	38	78	105	78	109	109	no	ac't.	299
1851.	Yellow fever.....								10	68	221	no	ac't.	741
	Miasmatic fevers.....	51	60	*44	†39	63	†11	*35	*59	*76	*62	61	†35	596
1850.	Yellow fever.....	1		2		1			4	62	33			103
	Miasmatic fevers.....	56	37	39	28	26	32	32	68	205	144	42	43	752
1849.	Yellow fever.....							1	17	214	416	112	9	769
	Miasmatic fevers.....	40	33	62	39	48	47	44	93	70	62	43	42	593

\* One week missing. † Two weeks missing. ‡ Three weeks missing.

The following memorandum was extracted from Dr. Fenner's Southern Reports, and was derived from the cases occurring at the Charity Hospital, and is further corroborative of the same.

1849.	Yellow fever.....							2	28	374	520	130	6	1,066
	Intermittent fever.....	109	114	138	117	69	155	368	592	763	720	360	684	4,439
	{ Typhus, typhoid, Re- mittent, bilious con- tinuous. }	91	143	225	163	164	180	160	191	285	142	101	115	1,910

Thus, in precise accordance with these principles, in parts of the country where prevails the less severe forms of these fevers, the causes do not exist in such an aggravated degree as in low grounds, the estuaries of

rivers, the outskirts of cities; but where they exist in an eminent degree, in hot, humid weather, in parts of cities where there is concentrated the most filth, in dark, unventilated alleys, in crowded rooms, where human offal, the worst poison of man, is accumulated, and where the habits correspond, here exists the most malignant forms of fever. These *very spots* are the birth-places and abiding homes of YELLOW FEVER; every body, who will take the trouble, may see it in these places every year; they are the very places which give birth and prevalence to it in all cities liable to this form of fever. They have been specially pointed out in the report.

Why these effects should not always *ensue at once* from exposure to these conditions, is no more known than why some people never take the fever at all, nor than having had certain diseases once, we are no more subject to them. But we do know that TIME is an important element in the causation of disease, and that susceptibility varies with the physiological condition. But who shall say we shall never know them in an investigating age? who shall set limits to the progress of knowledge? only those who never take the trouble sincerely to inquire.

Again, precisely the same exhibit is made in relation to Charleston and Savannah, as their records, now before me abundantly prove. Whenever a cause exists to aggravate the sanitary condition of either of these places, yellow fever occurs. Accordingly, the draining, paving and other sanitary reforms in Charleston, have made that one of the healthiest of American cities, subject to occasional epidemics, during very remarkable seasons, from some hygienic remissness. The aggravations in relation to Savannah, since the great improvement in her health, from the adoption of the dry culture instead of the wet, for rice, have been owing to special causes—some of which have been pointed out. Savannah, although having some cases of yellow fever every year, has particularly suffered from three very fatal epidemics, viz: that of 1817, '20 and '53—during these the additional malignity (certainly during two if not three of these occasions) were imparted to the disease, by cutting down and leveling the streets, spreading the refuse and offal of the yards and kitchens on the streets, and otherwise disturbing the soil by digging trenches for gas and water-pipes, and filling up squares and lots with fresh earth.

Egypt—the Campagna or Pontine marshes—Walcheren and Chagres, have each their peculiarities, but afford no argument in exception to the principles laid down, as I proceed to show.

The causes of the diversity of the types of diseases of different climates, medical investigation has not yet fully developed. Of that large class denominated fevers—the main outlet of human life—varying in the estimate of eminent men, from one-fourth to two-thirds—the mystery may be more nearly solved than is now generally imagined. The Plague in the East, the Yellow Fever in the West and the Typhus Gravior in England, are, by *general consent*, at the head of their respective classes, in these several great ranges of country. These climates differ essentially, not more in their temperatures than in their hygrometric properties, and in the mode of living of their respective populations. The climatic details are too limited in relation to Egypt to apply, fully, this mode of accounting for the plague, specially there. Two facts are well known in relation to the influence of causes readily arresting it. 1st. It is speedily put a stop to by the prevalence of dry winds from the desert. 2d. It is drowned out by the supervention of the inundation of the Nile; an instance is mentioned where this was so remarkable, that five hundred less died of the plague, the day after an occurrence of this kind, than the day before.

The same principles apply to Walcheren and the Pontine marshes, the insalubrious condition of both derive their controlling influences from their excessive humidity, their temperatures are *known to be too low to produce the development of yellow fever.*

Although it is not entirely true that yellow fever is confined to seaports, as supposed, or places near the sea, yet it is uncommon for it to break out or spread much in the interior—nevertheless, it is well known, and experienced practitioners will bear me out, that sporadic cases do sometimes occur far in the interior, when aggravated conditions of heat, moisture and filth exist in adequate combination to furnish sufficient cause. It is then developed without farther difficulty or need of “seeds,” etc. Thus it has occurred at Natchez, Woodville, Bayou Sara and at other places on the Mississippi, near the gulf, and insulated places far in the interior, where it was absolutely *impossible* for it to have been conveyed or imported, none being in New Orleans at the time, nor as far as we know, within 500 or 800 miles. The very idea of “germs” of it remaining over a season or so, is too ridiculous for argument and only requires to be mentioned to be repudiated by all men of experience. Thus, then, the only prop the contagionists have had, has been knocked from under them, and the true and only explanation has been made upon scientific principles. From all the observations I have been able to make—of the cause of this notable ex-



emption, to the extent it actually does exist, it is clear to my mind, that it arises as much, and probably more, from the difference in the hygrometric properties of the atmosphere, than in differences of temperature, (of course in combination with the terrene.) This can only be proved by accurate experiments, and these have not been made.

It is to be deeply regretted that, at this enlightened day, accurate and extensive experiments have not been made with the hygrometer, thermometer, etc., in direct connection with their important bearing on human health and life. It is nothing new, I know, to attribute the origin and extension of malarious fevers to great heat and moisture, but it has been done in a very indefinite manner, and as often disputed, and no precision has been connected with it, and no principle applied, until of late. Many have denied this connection, because the precipitation has not been large enough *in their estimation* — being unaware of that worst condition — the hygrometric state of the atmosphere — which they have at the same time unknowingly admitted in the form of mould on leather, furniture, etc. Now, it is not merely gratifying to scientific curiosity to know that this property can be detected with philosophical precision, by instrumental observation, but it is a practical fact, of the utmost value to society, as upon a foundation of a knowledge of a cause of disease alone (and this as one of the most controlling ones particularly) can we build the structure of prevention. Sanitary laws, then, must here have their only rational origin.

In the discussion of the cause of the difference of the types and grades of fever, there is an important omission, which I will embrace this opportunity to supply. Nothing is more common, not only here, but in Mexico, South America, the West Indies, Savannah and Charleston, during sickly seasons, while yellow fever may be prevailing among strangers or the unacclimated, for the natives or acclimated, to be affected with a milder grade of fever, under the same exposures — they are often so similar in type, as to be almost impossible to distinguish between the two. It is the same with the Africans (their first season) when taken to different regions, where the yellow fever may be existing, although the yellow fever proper, hardly exists in Africa, but an equivalent malignant type of fever does, to which they are habituated. This occurs constantly, when approaching in grade, they run into each other, and interchange symptoms, according to susceptibility and treatment. This difference of susceptibility satisfactorily accounts for the diversity of effects in individuals exposed to similar influences, one having a very mild attack and the other one of great feroc-



ity, without its being at all necessary to attribute them to *two distinct poisons*; the *yellow fever in the stranger being equivalent to the milder grade of periodic fever in the native*. It is precisely similar to what occurs in a man accustomed to indulge in ardent spirits or opium, an ordinary quantity or dose, has little or no influence on him, while on one not so habituated, a real toxical effect is experienced.

This difference of susceptibility, also varies in the same individual at different periods and from different causes. We often see a man pass through one or more, nay, through many yellow fever epidemics, in the closest and most intimate intercourse with the sick, and yet with the most perfect immunity, and in a subsequent season fall a victim to it. It is then, during the existence of an epidemic, the rule of prudence, sedulously to avoid committing any act of imprudence, that can *unbalance* the constitution during its prevalence—such, for instance, as a debauch, a fit of passion, a fall from a horse or carriage, a sudden fright, etc. I knew an instance some years since, where near a dozen young men, who during their first year had escaped the prevailing epidemic to an advanced period of the season, and who determined to celebrate their triumph by a feast, which terminated in a debauch; in the course of a couple of weeks, there was but a single survivor, and he was an invited guest and acclimated.

But there are direct, opposite and beautiful analogies in our profession to prove that the same poison or agent may produce diversified effects on the same individual, that is, act on *different organs*, in different quantities or doses. For instance, a small dose of opium exhilarates, a large one produces cerebral congestion, a small dose of arsenic strengthens and fattens, with a slow undermining of the constitution, a large one kills in a few hours, more or less; a small dose of ipecac or antimony sweats, a large one vomits; chamomile sweats, vomits, or acts as a tonic, dependant upon its mode of administration; and how numerous the influences do we expect from mercury, dependant upon the quantity and mode administered. Fluctuations in the weather, and particularly hygrometric changes, produce catarrh, pleurisy, pneumonia, and sometimes various intestinal-gouty and febrile affections, dependant upon the amount of exposure and individual susceptibility and predisposition. The balmy air which sustains our being, the purling brook which furnishes the pabulum for all animated nature and the deadly poison, have the same atomic elements, limited in number, but diversified in combination, which produce such different effects. But why multiply examples, which are absolutely numberless, to prove a truism in

medicine? Is there any plausible reason why there should be required a difference in the *nature* of a cause productive of fever, while a medicine, merely by a difference in quantity, should have such a diversity of effect? There is certainly none. It is in the one case as in the other, different portions of the organism are assailed *by virtues inherent in the dose*, or amount of poison, and such an interpretation is consistent with all we know in medicine and in nature. This is most aptly illustrated in the mode and rank of the organs on which the pathological influence is displayed, and thus admirably corresponds with the apt analogies just furnished. Thus the one attacks the more vital structures—the citadel of life—the brain, the sanguiferous system—those of cerebral life; the other, the *instruments*, the organs by which the body acts and continues existence, the branches—the outposts, as it were—the liver, spleen, mucous membrane, which by reaction, sympathy and symptoms, interpret the place and character of the attack, and call for treatment *through indications*. In the first, time is not allowed for this effectively; in the second it is; and it is through a long course of actions and reactions of and on the organs, showing the great difference between them.

But there is another proof, well known to medical men, which beautifully illustrates my position, although it has been most unfortunately used as an argument against it. It consists in the liability to attack; in the case of bilious or periodic fever, a second or a third time, or more, in fact, the oftener it is endured, the more liable to its repetition! This arises solely because the *organs* become more and more crippled at each subsequent attack. This is not so in yellow fever, because these organs are rarely embarrassed by the disease. It is the higher range over which it passes, and when the system is not entirely overthrown, recovery is apt to be rapid, thorough and perfect, and a remarkable renovation of the system is often known to result from it.

Let us apply these remarks and illustrations to the subject before us. Yellow fever has been denominated in the report, the highest and most malignant grade of fever known in the Western hemisphere, the proof of which is, that its mortality is much greater; so deadly are its attacks at times, that the patient succumbs in a few hours; sometimes the first symptom is the fatal black-vomit; hæmorrhages occur from all the mucous surfaces; at an early period the fatal look is exhibited, and the patient is walking about *actually dying*; there is not a pain or a symptom, (properly so called,) the sympathies connecting the system are dissolved, the fatal

blow has been struck at the centre of being, and man's majestic structure is in ruins. Now, this worst form of febrile disease occurs precisely in the seasons and places, where in the concurrence of all experience and reasoning from data thus furnished, we should a priori expect it to occur, viz: in the filthiest cities, where the least attention is paid to sanitary police, and in those very spots, places, houses and alleys of those cities, which are filthiest, most crowded, and the inmates of the worst habits; and it occurs just in those seasons and parts of seasons, when these effects are most calculated to have their worst influence on the human body, viz: in the hottest and moistest seasons; and if there is any want of uniformity in these outbreaks of yellow fever occurring where all these filthy materials are present, it solely arises because the other constituent, (the second blade of the shears,) the meteorological condition, which is either absent or defective. Here, then, is the combination necessary to render effective the poison productive of the *highest grade* of fever—*yellow fever*. On the contrary, the other forms of miasmatic and periodic fevers occur, when these excesses do not take place, in the same exaggerated degree. It is then the causes (or poison, if you will,) being in less force, the minor organs and instruments of life become assailed, and symptoms, which are their interpreters, direct to the local action and attack; now we see the liver, the spleen, the gastro-intestinal mucous membrane, the system of organic life, to bear the onus; time is allowed for reaction, and the struggle is made by the system to resist the disorganizing tendency on the special organ. The "Chagres fever," the jungle fevers of India, the bilious and marsh fevers of our own country, satisfactorily illustrate the action of all these secondary influences, and all most strikingly show the analogy between the causation of different classes of fevers with the effects before pointed out, and the influences of medicines on the system just stated. Can demonstration be clearer?

A high dew point (may be, with other aid) will produce bilious fever, by acting on the materials of the blood and the secretions, and thus on the *instruments* of life; but for this latter (yellow fever) it requires a concentration of these agencies with others, to give intensity, and thus as we have different effects with medicines, according to their dose, so it is with these agents, and yellow fever is the result. This is not mere speculation, nor does it depend upon analogy alone, but it has every presumption in its favor, from the actual occurrence of the disease (yellow fever) under circumstances where this exaggerated condition alone exists. Can stronger proof be required?

It is easier to account for the difference in the types of fever on the ther-

mometrical or latitudinal than on the longitudinal scale. All medical history informs us of the geographical limits of fevers, that where the temperature is high or long continued, with a great amount of moisture, they increase in malignity, (other things being equal,) as these diminish, they lose their severe type or grade, until they finally cease as we approach the arctic or antarctic poles; and not only fevers, but disease almost disappears, and navigators visit, remain months and years, and return from those regions without scarcely losing a man. But it is more difficult to say *why* yellow fever should be at the head of its class in the West and plague in the East. There are climatic and hygienic peculiarities that are still unexplored by which it may be explained, provided we seek for them in the true spirit of philosophic research, and experiment perseveringly with the means science now furnishes her votaries.

In relation to our great Western disease, yellow fever, early history has not furnished us with many more valuable facts, as to its *causation*, than it has of the plague. If yellow fever is an American disease, it was not found here on the discovery of the country, it was only *developed as a climatic influence on European constitutions*; it was only after these Western regions were occupied for more than a century, that it began to prevail; when the settlements became more or less dense, and men congregated in cities, population became crowded, the habits of colder regions were transplanted where they were so unsuitable, and tropical hygiene not understood, that it had its birth.

But science has dawned in the West in the awakening of the mind due to a new era: the spirit of the age now expects to know the *cause* of every thing, secondary agencies are the hand-maids, the interpreters of the will of Deity; it is only upon this foundation can the *true principles of prevention* (or sanitary laws) be based. It is a law to which all that is dear to man is subject, that as there is no fixed, stationary position for man or for science, the moment we stop advancing we are retrogressing. Let us then, fully advised, use all the means which science, still in her infancy, has amply supplied her votaries, and interrogate nature with the honest and sincere desire to arrive at the truth, instead of speculating with a prurient imagination upon the half-stated and the false facts with which imperfect tradition furnishes us, and we shall thus sooner unravel the mysteries which environ this hitherto dark subject.

Scientific investigation has furnished us valuable data to begin with. I have just stated why yellow fever never occurs in the Pontine marshes, and was unknown in all the ravages at Walcheren, the average temperature



being under 80 deg., below which, it is now known, yellow fever cannot originate. It has been shown that a summer temperature of 60° is necessary for the production of *fever*, and that it never appears as an epidemic, unless the temperature reaches 65°. These temperatures are reached at Walcheren and the Pontine marshes, but not 80°, and for the existence of yellow fever, *this temperature* must not only be reached but *endured weeks if not months*. So precise has science now extended its investigations, but we trust, it is only at the threshold. We now know why yellow fever would not spread in Charleston when carried there in May and October last, and why, being taken to Aiken and Columbia, at a more advanced period of the summer, it would not spread either, and that when taken to Blackville and Augusta, it did spread. In the first cases there was a deficiency of heat and humidity, etc., while in the latter they existed. The condition of Augusta has been noted in the "Introduction" to the Report, that of Blackville is low, with a pond in its midst imperfectly filled, swamps surround it, and the irregularities of the ground have been filled up with offensive putrifiable materials. Of the various occurrences of the many "spontaneous cases," mentioned with such emphasis in the Report—in the absence of precise observations with the thermometer, there are other proofs mentioned, of the existence of, at least, these two agents, as well as others, and I refer, as ample proof of it, to the many "spontaneous cases" of the fever mentioned in the Sanitary Report.

And, again, I repeat the fact, which defies contradiction, that when the temperature and humidity are lowered to a certain degree, (stated) yellow fever, as an *epidemic* ceases in this latitude. These are, I believe, irrefutable truths, constituting the laws of the disease. Another is no less certain, that without the concurrence of the TWO AGENCIES mentioned in the Report—no yellow fever has ever occurred, nor, by sequence, ever can occur! No amount of *heat* and *moisture alone* has ever or can ever produce it. No amount of *filth alone*, can effect it; when concentrated, it may produce asphyxia and death;—when less, with defective ventilation, crowding and a low temperature, it may produce the worst forms of typhus and other fevers, but *never yellow fever*.

I have said that TWO CONDITIONS are required to coalesce or combine, in order to produce the alleged effect—the meteorological and the terrene—and that this latter consists of filth or decomposable organic matter of all kinds—of which I consider fresh rich original soil to be an equivalent. I have never said or believed that one alone was sufficient—yet against ONE ALONE has all the force of opposition been expended!

We think, then, that the following propositions have been clearly demonstrated, from the facts, by experimental observation and by every principle of fair analogy, viz :

1st. That ordinary mud — consisting of the clay and sand deposit of the River Mississippi — is different from the “original soil” referred to in the Report, in this, that one has organic matter in it, and the other has not, and that it is farther mixed with every species of decomposition, and particularly, with the offals of society; that personal excreta, of all kinds, constitute the worst forms of organic matter; that yellow fever results from these, in combination with the meteorological ingredient, and that hence, it is the highest form of fever, and occurs mostly where these exist, in greatest excess, as in the filthiest parts of crowded cities.

2d. That yellow and bilious fevers proceed from the same causes, although differing in degree and amount.

3d. That these causes, acting upon individuals of different susceptibilities, (as the acclimated or native and the unacclimated,) produce these different effects — in the first, developing a milder grade or periodic fever, and in the second, the aggravated form, or fellow fever.

4th. That the main pathological cause of the difference in the phenomena exhibited in yellow fever from bilious fever, arises from the difference in the rank and importance of the organs attacked in each case respectively — in the first it is on organs whose integrity is more immediately essential to life, as the nervous and sanguiferous systems, or those of cerebral life; and in the second developing its influence on subsidiary organs, or those of rather secondary importance — those of animal life, as the liver, spleen, stomach, etc.

5th. That these causes proceed from all the circumstances that impair the purity of the air, which is essential to healthy existence, proceeding from vegetable and animal decomposition of all kinds, and disturbances of the original soil — that these, in the aggregate, constitute **MALARIA**, together with certain meteorological conditions, which are indispensable, to give it activity.

6th. That all we know of contagion, is, that being a specific virus, the product of secretory action, *it must be*, in its very nature, independent of all these circumstances and conditions; the existence and the spread of these can necessarily have no connection with it. But, as all the conditions productive of vitiated or bad air must tend to extend the above influences, within the area of that impure air, and in proportion to that impur-

ity and the meteorological condition, so the susceptibility to the spread of these diseases will exist.

7th. The final proof of all these propositions is, that when the conditions above pointed out are removed, or no longer exist, the effects cease, *causa sublata tollitur effectus*.

Throughout this paper I have endeavored to impress the reader with the firm belief which has pervaded my own mind—that as there can be no effect without an adequate cause—so all fevers, and at the head of them particularly, yellow fever, must have some adequate cause for its production, and I solemnly entertain the firm and abiding conviction, that we are not ignorant of that cause. Our author attributes yellow fever to “accident or specific causes.” The term “accident” may be applicable to man’s action—but not to the Creator’s—with him, be it reverently spoken, there can be no “accident.” In its reference to us, it only means our ignorance of a cause. But herein I have shown that this is a most egregious error, and that we know as much of the cause of yellow fever, as we do of any other ailment with which man is afflicted, and that, moreover, there is nothing “specific” about it. Nay, it is not too much to hope, with the better understanding of climatology, with the clear proof, now well known to every well read medical man, that fevers of every class and type have their geographical limits, and this will be the more precise, as both of these are better understood, (i. e., fevers and climate,) that the great mystery of the proximate cause of fever, that is, the *ipse morbus*, will be, ere long, unravelled. To hasten that long coveted period, to make this of real practical value, (its only use) we must push on anew in our studies of climate and its relations, for it is upon a thorough understanding of all these, as a foundation, that we can erect any rational structure of sanitary and preventive measures. This study must enter into medical education in the legitimate orthodox way, through a proper system in the schools. Let it receive its earliest impulse from this first Sanitary Commission ever instituted in America to investigate the origin of epidemic diseases, and if we shall have succeeded “in establishing a single principle in our science”—we shall, in the language of our eminent countryman, Dr. Rush, “have done that which will lead to more truth in one year, than whole volumes of uncombined facts will do in a century.”

But it is time to bring these observations to a close, and before doing so, I will embrace the occasion, for the first time, to speak a few words of myself, and it is solely in self-defence, humiliating as it is, that I am now

urged to do it, by falsehood and censoriousness. It is false that I ever assumed any position or rank in the sanitary commission that was not *prof-fered* me by its intelligent members. I need not defend the purity of my motives before an audience of scientific gentlemen like the present, with a knowledge of my long and arduous labors for the public, without fee or reward. The charlatans of science, the "the Trays, Blanches and Sweet-hearts" of the purlieus, may bark as they have ever barked at me—my course is onward, it has for its end not personal aggrandizement or newspaper reputation—it has emblazoned on its banner the solid advancement of our city and the good of society; it does not pander, by "Buncombe" writings and speeches, to the worst prejudices of the ignorant, or the time serving of the unprincipled; it shows the facts, the painful facts, on which I have based all my opinions, the records of near half a century, as the sole basis of necessary sanitary reform; I have shown that these effects are in precise accordance with all we know of causation in the history of disease, that these disastrous results have ever, and must ever, ensue from the state of things around our city, *until corrected*; but, above all, not only is the cause of all our physical calamities pointed out, but from these facts principles have been deduced, from which have resulted the REMEDIES for them, and here, thank God, the distinction between our opponents and myself is most marked; here they have, at least, shown their consistency in *never advising any thing to improve the sanitary condition of New Orleans*. For all our physical ills, I have for more than twenty years repeatedly pointed out the curative means to remove them, being convinced from the records and experience of other places, and seeing the influence of concentrated local vitiation here, that they were perfectly susceptible of amelioration, I have not ceased, in all that time, to urge the necessary means upon the public authorities in spite of abuse and depreciation of every kind. This is no more than was to be expected. I have been supported in it by the dictates of my own conscience—by a few personal and scientific friends, and by intelligent people everywhere abroad; I have an abiding faith in the sense of justice of our countrymen, and although long postponed, that the day of appreciation will come, with healing on its wings, and that the toils and anxieties I have endured to promote the sanitary advancement of our city will be acknowledged. I have the moral courage to continue that course, whatever may be the consequences to me personally, to stem this torrent of public prejudice and ignorance, arising mainly from the efforts of these writers. And who has dared to stand forward, before this public, to defend the right, but Drs. Axon, Fenner,



Simonds and myself, and the late sanitary commission? Now, all that we have said, is either true or it is not. We think it *has been proved*, as our only surety for the future, in defiance of every possible contradiction or misinterpretation, and if so, it is time the onus should be put upon the shoulders of those who ought to bear the blame of it, and the responsibility, for all the disastrous consequences which have resulted to our city from evil counsel is a heavy one. It is second only to that of him, who attempts to unsettle the faith of the devout christian in his religion, and deprive him of his only hopes of happiness hereafter, as it does our city of the most efficient cause of its prosperity. We are very far from laying this responsibility to the door of the author we have commented on, for we know that a better heart beats in no man's bosom. But there are others without his purity, who know or ought to know better—who, with the necessary information more directly before their eyes in practical effect, oppose reform, solely from its originating with others. To such, I have no answer. All the facts are now before the public — and there is one thing perfectly clear and unequivocal, that the lives of thousands, the interests of millions, the prosperity of the city, its reputation at home and abroad, and the public opinion of the civilized world, all unite to urge upon the public authorities, without loss of time, before there shall be any more sacrifices, to carry out, to their fullest extent, the measures recommended by the sanitary commission—as being those most conformable to the advanced and enlightened spirit of the age, and identified with the best interests of the city and the public welfare.



## Wounds of the Heart and Pericardium.

By J. HANCOCK DOUGLAS, M. D., of *New York*.

The late tragic affray which has produced so great an excitement throughout our city, and stirred up to an unusual activity the dispensers of retributive justice, has also given to the medical philosopher a problem to solve, and to history another instance of anomalous injury of a vital organ with life prolonged to a limit almost without parallel.

In default of other information, we take from the *Tribune*, of March 10, the result of the autopsy held over the body of Wm. Poole by the coroner, as follows :

"A post-mortem examination was made on Wednesday by Dr. Finnell, in connection with Drs. Carnochan, Putnam, Cheeseman, Hart, Wood and others. They found two wounds on the surface of the body—one in the lower and outer portion of the thigh, the other in the chest. The one in the thigh had two openings about an inch apart, and measuring a quarter of an inch in diameter. It passed through just beneath the skin without touching the muscle of the leg. The ball in the chest entered the sternum just at its junction with the cartilage of the fifth rib, passing through the bone and pericardium into the substance of the heart, where it was found. On raising the breast-bone and exposing the pericardium, it was found very much distended, measuring five inches in its transverse diameter, and six in its vertical. It contained about thirty ounces of a sero-sanguineous fluid. The external surface of the heart was covered with fibrinous exudation, the recent product of inflammation. The heart was washed and laid aside with no suspicion that the ball was lodged in it, until after two hours search in the cavity of the chest, and especially along the side of the spine. At last the heart was very carefully felt over, and the bullet was found embedded in its muscular texture. On making an incision it was exposed. Its lodgment was in the septum, between the ventricles, about an inch and a half from the apex of the heart, and a quarter of an inch from its surface. The muscular substance had united over the ball and healed so far that the point of entrance was obliterated. He lived for twelve days without any palpitation, or any fainting or syncope such as is usually experienced in a morbid condition of the heart. Its action was perfectly regular. There is no question but that, under favorable circumstances, he might have recovered, and experienced little, if any, inconvenience from the ball. Four or five days after he was shot, he was quite strong. A story goes that a man came to see him, with whom he had had some difficulty, and asked him how he was getting along, whereupon he jumped out of bed, and said: 'I ain't dead yet; I am well enough to flog you yet.' He died from effusion in the pericardium, stopping the action of the heart. It is probable that, on Wednesday night, the effusion began to come on, and it rapidly increased. At 9 o'clock, on Thursday morning, it suddenly increased, and he sank immediately. The lungs were pale and œdematous. The liver, kidneys, stomach, and other organs presented an unusually fine view of organs in a sound condition. The whole body was a most perfect specimen of fine muscular development; even to the ends of his toes the muscles were remarkably developed."

In this account we notice the following pathological conditions, which particularly attract our attention:

The wound of the pericardium by the entrance of the ball.

The size of the pericardium and the amount of fluid contained within it.

The fibrinous exudation upon the surface of the heart.

The presence of a ball in the septum between the ventricles, an inch and a half from the apex, and a quarter of an inch from the surface of the heart.

The obliteration of the wound of this organ by union of its structure, so that its point of entrance escaped notice, till carefully examined.

The presence of a foreign body in the muscular substance of the heart for twelve days, without producing any symptoms of discomfort, or in any way obstructing the functions of the heart.

The partial and apparent recovery, and afterwards sudden change and rapid death.

In reviewing these, we shall examine such cases as are at our command, and which bear any resemblance to the case under consideration, or which will elucidate any of the pathological points we have enumerated above.

All the ancient authors consider wounds of the heart as being necessarily mortal. Hippocrates enunciated this in his 18 Aphorism; sec. 6.; and Galen, in his Commentaries on this Aphorism, admits that wounds of the heart are always fatal. Celsus, (Lib. v., cap. 26,) among injuries of organs enumerated as incurable, mentions those of the heart; while Paulus, Egineta, and other early writers, only reiterate this assertion. Van Swieten, however, (Com. on Boerhave's Aphorisms, Edin. 1776, vol. II., p. 70,) says: "That the ancient authors seem to have said this rather from hypothesis, than from real facts or experiments," and asserts, (p. 73,) "that all wounds of the heart are not mortal, since they are very different according to the several parts of the heart in which they are inflicted." Yet Sir Chas. Bell, in these latter days, in the edition of his work on the Institutes of Surgery, as late as 1838, says: "As to the wounds of the heart and great vessels, I need not fill my book with narratives; they are fatal wounds."

Modern writers have, however, found in comparative anatomy, and in a very few instances in the human body, examples opposed to this view of our fathers in medicine. The fact related by Weber of a ball found in the heart of a stag, and another similar instance, where a doe was killed, in whose heart a ball was found encysted, weighing 292 grains, with numerous other recorded facts of animals, such as the wild boar, deer, dogs, etc., having carried for a longer or shorter period, either balls or some other instrument used in hunting, in their hearts, are evidences that serious injuries can be inflicted upon the heart without producing instant death.

M. Richerand observed upon the body of a man, who, during life, received a wound from a sword in the chest, a cicatrix in the pericardium and heart, and M. Velpeau found a cicatrix represented by a fibrous line through the whole thickness of the right ventricle, which had been wounded a year before by a cutting instrument. (Arch. Generales de Medicine.)

M. Larry presented to the Academy of Medicine, Dec. 13, 1831, a person wounded by a ball in the chest. It entered two or three lines below the left breast, and passed out between the vertebral column and the scapula, a half inch from its inferior angle, and in its course it probably passed through the pericardium, a part of the left lung, and touched the surface of the heart. From the symptoms he was expected to die, but recovered. The respective situations of the cicatrices and the anomalous beatings of the heart presented to the ear, justified the diagnosis. (Arch. Gen. Fev., 1831.)

There are cases of severe wounds of the heart and pericardium with recovery. While numerous instances are on record where the heart has been wounded, and the instrument producing the wound remaining in situ, life was preserved for various periods, even as long as nine months, yet death supervened from subsequent inflammation.

In the New York Medical Examiner, Vol. V. No. 2, et S. p. 94, we find a dissection of a negro recorded, in whose heart was found a broken needle imbedded in the external wall of the left ventricle, its point towards the apex of the heart, while the broken end was in one of the columnæ carnæ; and in the same paper is quoted a fact related by Dr. Learning of a seamstress, who, having thrust a needle into her chest, by falling against a table, died nine months afterwards, having suffered from pleurisy pericarditis and pneumonia. The needle was found partly in the wall of the right ventricle, partly in the septum, while the point projected a quarter of an inch into the left ventricle. Medical journals abound in instances similar to those occurring in both animals and in the human race.

Wounds of the pericardium are not necessarily fatal.

South in his edition of Chelius' Surgery says: "Wounds of the *pericardium* sometimes occur without injury of the heart, and may be fatal or not." To support the negative he quotes "a case related by Hennen of a bayonet wound of the pericardium and diaphragm. The patient recovered of the immediate effects of the injury and died three months after of pneumonia."

Stokes, (Dis. of the Heart and Aorta) relates the case of a gun shot wound of the chest, where certain sounds were heard which induced him to believe the pericardium had been injured. "The best idea of these signs may be given by stating that they consisted in many distinct points of friction sound, each of which though extremely circumscribed, conveyed the impression of a resisting or cartilaginous deposit. Little doubt could



be entertained that the pericardium was injured, while the inflammation instead of spreading over the entire surface, was confined to the points of lesion."

Wounds of the pericardium, complicated with those of the heart and lungs, are also curable. The cases we have already cited, reported by Messrs. Richer and Velpeau, show cicatrices of extensive wounds of those organs with entire recovery. In addition to these we have an instance presented to the Academy of Medicine, Paris, by M. Bongon, of a man who died at his hospital having scars of an old penetrating wound of the chest, the lungs, pericardium and heart had been wounded. These different organs were cicatrised, and yet the patient died of a disease quite foreign to the wound. (*Arch. Gen.* Jan. 1825.)

Puncture of the pericardium for effusion into this sac, has recently been performed without danger to the patient, and with great success in the treatment of the disease for which it was performed. But we shall speak of this hereafter.

The pericardium is a closed sac, investing the heart, and permitting a free exercise of the movements of that organ by its smooth lubricated surface, as do the pleuræ the lungs. It is not much larger than the heart itself, and ordinarily does not contain more than a half ounce to an ounce of fluid; yet in this report we find thirty ounces of a sero-sanguineous fluid between the serous membrane of the pericardium and the heart. This sac was distended so as to reach the limits of five inches in its transverse diameter and six in its vertical. The heart, in its normal condition, has, according to the measurements of M. Bizot, 4.2,430 inches in its transverse diameter and 3.8,299 in its vertical diameter, in the male adult. Nearly a quart of fluid was contained then in the space included between these two measurements. Such an amount must have produced all that series of symptoms which are so distressingly apparent in pericarditis with effusion.

We conclude, from the absence of any mention to the contrary, that the external wound must have entirely closed, for otherwise this effusion would have found a passage out, and the heart would not have ceased its functions from this cause.

What produced this effusion? Did it take place gradually from the moment that the wound was received, or did it arise suddenly from some other exciting cause? We learn that it was not suspected till eleven days after the wound was made, and then accumulated so rapidly, that in about

fifteen hours its work was accomplished. Nor is there any evidence given that the effusion took place slowly. There is no mention of slight dyspnoea daily augmenting, or any pain or change in the action of the heart. On the contrary, it is distinctly stated that for "twelve days there was no palpitation, fainting or syncope, such as is usually experienced in a morbid condition of the heart."

The fluid effused was sero-sanguineous, which can easily be accounted for by the nature of the wound. The greater proportion was serum, the result of inflammation, while the blood mingled with it was the loss sustained at the moment the injury was inflicted, and must have been slight in quantity, as it produced no disturbance, as before mentioned, in the functions of the heart. Stokes says: "That an effusion can exist in moderate quantities without producing pain, or excitement of the pulse or heart, or increase of dullness over the heart."

The effusion was the result of subsequent inflammation, produced either by the wound of the pericardium, by excitement, or by the rubbing of the fibrinous exudation, found upon the surface of the heart, against the serous surface of the pericardium. We do not know that this exudation was general or confined to the limited surface of the organ at the point where the ball entered. It at least had the effect to close the wound against further loss of blood, and was one of the means whereby the wound was concealed.

Reasoning hypothetically, we should instantly yield our belief to the statement of the ancient authors, and say with them, that such an injury to the fountain of life would be instantly fatal. But here we have an instance to conquer our belief, and we have already enumerated other instances of wounds of the heart, both in men and animals, with partial and complete recovery. Van Swieten mentions many like facts, and in the *Dictionnaire de Médecine* are recorded several similar instances. In the *Archives Generales*, of September, 1839, M. Jobert has embodied, in a paper upon wounds of the heart, the outlines of a case which very nearly resembles the one which has induced these researches. Latour found at the autopsy of a soldier, who died from an affection quite distinct from a wound he had received many years before, a ball embedded in the right ventricle, near the point of the heart, and in part covered by the septum medium. This soldier at the time he was wounded, was taken up almost dead, while a severe hæmorrhage caused those attending him to anticipate death at any moment. However, by assiduous and well-directed attentions, the blood

was arrested, the wound suppurated, some splinters of bone from a fractured rib passed out; three months afterwards the wound cicatrized, and the patient only felt slight palpitations, which continued three years, when he died, and, at the autopsy, this was explained by the presence of a foreign body in the substance of the heart.

In the autopsy of Poole, the ball was found in the septum, an inch and a half from the apex of the heart, and a quarter of an inch from the surface. This was the most favorable point for the lodgment of the ball, and corresponds as near as may be to the spot where the ball was found in the case reported by Latour. The precise course of the ball is not clearly indicated, but it is more than probable that it passed through the structure of the right ventricle, as the greater proportion of cases of wounds of the heart occur in this part from the position of the organ. They are less fatal, too, than wounds of the left ventricle, for the action of this side of the heart is less energetic, as the lungs are ever ready to receive the blood, and the auriculo-ventricular valves of this side permit a reflux of blood, while the left ventricle labors continually to force the current of blood through the vessels, so that in the one case there would be less liability to excessive hæmorrhage into the pericardium, and a greater chance for the formation of a clot, while in the other, the blood would flow out at each systole of the heart, preventing the forming of a clot, upon which the recovery depends.

The thickness of the septum at its middle, which would very nearly correspond to the point indicated above, is, according to Bizot, 0.4,362 of an inch—nearly half an inch—giving abundance of room for the lodgment of a pistol-ball.

The presence of such a body in the structure of the heart it would seem would produce excessive agonies, yet these two cases are proof to the contrary. The heart is not a highly sensitive organ; it can be irritated by instruments, pricked, wounded, and experimented upon, as has been repeatedly done, both in men and animals, without causing any expression of pain. It is, therefore, difficult to determine positively, in penetrating wounds of the chest, whether or not the heart be involved, and it is not easy to diagnosticate the existence of an effusion into the pericardium, unless it be in large quantities, or to distinguish it from pleural effusion. There is a great variety of symptoms, but none constant or strictly pathognomonic. Upon this point all treatises upon the subject are diffuse, yet M. Jobert, in the paper already referred to, considers as a pathognomonic

sign of wound of the heart, the existence of a loud hissing, resembling that which is produced by the passage of blood from an artery into a vein. M. Ferrus had previously noticed this sound in one case, and M. Jobert discovered it in three others. Depending upon the flow of blood, it would be wanting as soon as the hæmorrhage ceased, or a clot was formed in the wound.

A remarkable fact, in connection with the case we report, is the absence of any trouble in the movements of the heart or pulse. The case we have mentioned bearing a close resemblance to it, that of Latour's, palpitation persisted until death, and in the record of all cases we have examined, this sign existed to a greater or less degree, yet being observed in other affections of the heart, it can not be considered as a sign of this lesion.

We have seen, then, from the recital of cases, that neither a wound of the heart, the presence of a foreign body in its structure, nor any injury of the pericardium with slight effusion, need necessarily produce death. What then was the cause of death, when eleven days had elapsed after the wound was inflicted, and the patient was considered out of danger? Jobert says, that one of his cases, under the influence of a powerful emotion, was suddenly taken with violent pericarditis, from which he died, when there was every reason to hope for a speedy recovery. We learn that Poole was, at one period of his illness, highly excited, and soon after the effusion took place. And yet this effusion was not as large as has been repeatedly found at post-mortems. Bouchet gives one case where two litres, more than two quarts, were found; Louis, four litres, and J. Frank six litres. In these cases the pericardium was distended to  $7\frac{1}{2}$  inches in height, and 9 inches in width at its base. (*Dict. de Méd.*, vol. 23.) These show how much pressure the heart can support by being gradually submitted to it. Dr. Stokes says, that "too much importance has been attached to the effect of pressure by the superincumbent fluid. It is singular how much pressure the heart is capable of bearing without any important disturbance of its functions." Influenced by this remark, should we not look for some other cause which might have accelerated death? Had not the fibrinous exudation an important action in producing this result, by binding down the heart, paralyzing its action, and particularly impeding or checking the action of the left ventricle? Should we look upon the effusion as the sole cause of death, or view it as an accessory?

In closing this review of this fatal case, which has so much interested all classes and professions, we would suggest that the question asked above,



might have been solved by the operation of paracentesis, and if death was caused by the effusion alone, then another means of restoration would have been adopted.

This operation has been frequently performed with success. Dr. Romero, a Spanish physician, has performed it three times in idiopathic pericarditis with success, and Prof. Trousseau recommends the operation in a late number of the *Arch. Gén. de Méd.*, November, 1854, remarking, "That paracentesis of the pericardium does not produce any of those imaginary dangers, before which experimenters have so long recoiled."

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## Lead Poisoning Treated with Iodide of Potassium.

*By* DR. H. J. RICHARDS; *Reported by* C. J. BICKHAM, *Student, Charity Hospital.*

Pedro Sacolovichè, aged twenty-five, admitted April 11th, with acute symptoms of lead colic—the blue line upon the edge of the gum characteristic of the presence of lead, as distinctly traced as if pencilled with indigo. The patient admitted, upon questioning, that he had swallowed a musket ball by the advice of an old woman, about a fortnight previous; that it had entirely failed as a remedy for the habitual constipation under which he had been suffering; and that he had experienced uneasy sensations ever since. He was ordered to drink freely of flaxseed tea, to have a pill of Croton oil, opium, soap and rhubarb, every hour, with copious injections, until purging commenced, and to take five grains of the iodide of potash thrice daily in cinnamon water. The stools at first produced were hard and scybalous, but in a day or two, after omitting the croton oil and substituting the *ol. ricini*, as the bowels responded more easily to the milder cathartic, the stools became softer in consistence and of a bright yellow color. The same tinge characterized the urine of the patient, and this effect was attributed by Dr. Richards to the decomposition of some of the salts of lead—probably the acetate—by the iodide of potassium. If this chemical phenomenon actually occurred, the proportion of the iodide was in excess to the quantity of the salt of lead probably generated during the twenty-four hours, and the iodide of lead remained in solution. A chemical examination of the patient's urine would have verified the rationale thus set forth, and strengthened its application to the hypothesis

advanced by some authors on therapeutics on the efficiency of this salt in cases of lead and mercurial poisoning, in instances where the evidence of their presence in the system is less clearly proven than in the one now under consideration.

Under this treatment the patient was relieved of all his distressing symptoms, until the 17th instant, when he voided a musket ball weighing an ounce, less a few grains.

We have presented this case as illustrative of the hypothesis of M. Melsens upon the action of iodide of potassium in lead and mercurial poisoning; and as presenting some proofs founded upon clinical observations of its efficacy. The proofs are in this instance partly inferential, without chemical analysis to confirm them; but the final expulsion of the ball permits us to resort to chemical analogy, as some substitute for the more positive proof afforded by chemical analysis.

Pathological chemistry has established the facts that both lead and mercury have been found in different tissues after death. It only remains, then, to be proven that by entering the circulation the same phenomenon can take place between the iodide of potassium and the metallic base in combination with the tissues—that re-absorption shall occur—and elimination of the new compound be effected. This is a question of considerable practical importance, and we shall carefully note future cases that may still farther elucidate the matter.

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## Communicated.

By D. W. BRICKELL, M. D., of New Orleans.

*Messrs. Editors.*—Permit me to call attention to a subject, which is not only interesting, but, so far as I am aware, possesses some degree of novelty. Fully impressed with a knowledge of the great risk that a man incurs by laying claim to the *discovery* of any thing, however trivial, I by no means boldly assert my pretensions on that score; but must content myself with an honest avowal to the effect, that I have never heretofore heard my subject discussed by medical men, nor have I ever met with an item relative to it in my reading. Still, as I have never had a very enlarged intercourse with my professional brethren, and as my acquaintance

with "the books" is not the most extensive, what I write may, at last, prove an old story to many of your readers. But even should it turn out that I am merely restoring to light the fruit of some other man's mental labors, it will, at all events, be a source of much satisfaction to me to know that I have been instrumental in recalling to the attention of medical men a very important subject, which, I venture to assert, has occupied few, if any, of their moments of professional meditation.

The disease called *Lucorrhœa*, or *Fluor Muliebris*, is one which occupies a prominent position in the daily routine of the general practitioner of medicine; he meets with it at every turn, and is much more frequently consulted about its treatment than he would desire to be. Volumes have been, from time to time, written on the subject; it has been divided and subdivided into varieties, and learned disquisitions have been put forth on the subject of its treatment. Notwithstanding all this, however, I fear the sum total of our ignorance concerning its cure, would far outweigh any accurate knowledge we may have of it; and so true is this, that a majority of practitioners have an entire distaste for this disease, under all circumstances. On this latter account I anticipate but few careful and interested readers of the brief remarks I am about to make, and it is only to those few that I can look for future coöperation in the accumulation of facts and observations which may tend to prove or disprove the correctness of my position.

For several years past, I have directed my attention to the study of this disease, both on account of the great interest I have always taken in every thing relating to diseases of women, and because I was thrown in contact with some of the most obstinate cases, and sometimes in subjects whom I dared not to neglect, even if I were so disposed. It was in one of this latter class of cases that I first had disclosed to me the following interesting facts, and subsequent observations have altogether tended to strengthen my translation of the phenomena witnessed.

There is a class of females (high and low) who, notwithstanding they may be generally, comparatively, or even entirely free from *leucorrhœal* discharges, the moment pregnancy is established they become the subjects of the affection, and often in an aggravated form. In those, otherwise entirely free, the discharge during pregnancy will be moderate; in those habitually the subjects of the disease, there will generally be an aggravation of the symptoms, sometimes amounting to absolute torture and excessive prostration.

Thus far, my observations have been confined to five individuals; in two of these, (subjects ordinarily free from leucorrhœa,) the discharge has appeared regularly with four successive pregnancies each—the same promptly disappearing in each case with the completion of the process of gestation. In the three remaining cases, I have only had opportunities of observing the phenomena in one or two periods of pregnancy.

Were I to stop here, the matter might be regarded as a mere coincidence, and no importance be attached to it; but the most interesting fact connected with the subject follows. In one of these cases the leucorrhœa existed only during four or five days, at the end of each lunar month, or rather at the *regular catamenial periods* of the individual—reckoning from the period immediately preceding conception; in the remaining four, although there was a constant discharge, to a moderate extent, during the entire term of pregnancy, still, at *every* catamenial period (reckoning as above), there would be a distinct aggravation or increase of the same, accompanied by all the usual unpleasant sensations peculiar to the normal accession of the menses in the unimpregnated female. In such cases as were exempt from leucorrhœal discharge in the unimpregnated state, the flow not only ceased immediately on the accession of the first symptoms of labor, but never again appeared, until induced by the reestablishment of pregnancy. In the cases which were habitually subject to the discharge, there was a subsidence of the flow in its aggravated form, the patient merely relapsing into her old habit.

I am, by these facts, led to believe in the existence of a form of leucorrhœa *peculiar to pregnant females*, (though not existing in all), and constituting a variety totally different from all others hitherto described. Moreover, I regard the discharge as a positive type of the regular menstrual function, which otherwise manifests itself in the unimpregnated female, and which has not only been known to exist normally throughout the entire term of gestation, but is even said to make its normal appearance, in some instances, *only in pregnancy*. It is well known that, not only does natural gestation terminate in what would otherwise have been a regular menstrual period, but parturition itself is being regarded (and certainly not without very plausible grounds) as “*essentially a menstrual period* ;” moreover, it is, also, well known that in those pregnant females, who are disposed to abortion, each effort at this process is prone to occur at what, in the unimpregnated state, would have been a regular catamenial period. In fact, all the phenomena attendant on the process of gestation have



clearly the impress of periodicity. We cannot, then, be at all at a loss to conceive that a periodic fluor albus, from the mouth and neck of the uterus, may exist in some pregnant females as a substitute either for the regular menstrual discharge, or for those abnormal efforts at abortion which are so frequently known to exist—which latter efforts, however, I cannot help thinking Mr. Tyler Smith is right in regarding, as in themselves, merely exaggerated efforts at the continuation of the function of menstruation suddenly interrupted by the accession of pregnancy.

I do not propose to do more now than simply direct the attention of those particularly interested in the diseases of females to this subject; consequently I avoid a detailed discussion of the same. If it be true, that there does exist a variety of leucorrhœa peculiar to the pregnant female, it will readily be seen that much importance attaches to a prompt diagnosis of the same, in order to interdict forthwith a meddling practice, which must inevitably prove injurious rather than beneficial to the patient. For my own part, I am satisfied of the truth of the proposition, though I am aware that my data are not sufficiently extended to appear convincing to minds, even a little less than skeptical. To the candid inquirer, however, there will appear nothing unreasonable in the premises; all I ask is an honest search after the truth, and I trust, that those who have more ample field for observation than myself, will join me in collecting further data.

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## Excerpta.

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**OPIMUM IN THE TREATMENT OF CHRONIC ULCERS.**—*By Mr. Skey.*—Opium produces a certain effect on the chronic and callous ulcer; if it answers here, it will in other diseases, having the special characters of the chronic ulcer. The pharmacologists tell us it is a stimulant and sedative. What do you understand by the stimulating properties of opium? I should be at a loss to call to mind any cases in which it is given as a stimulant, unless in such as these. In nineteen out of twenty instances it is employed as a sedative, as for example, in relaxed bowels, and to promote sleep. I can show you it possesses properties quite equal to these: it prevents sleep sometimes; when taken continually it tranquilizes, but it will not always produce sleep. What is the action of opium to which I allude? Whether it acts on the centre or on the periphery, whether on the heart or capillaries, I do not know. But this I do know, that it promotes a healthy action on the capillaries of the body, and in cases of chronic ulcer, which I select as an example, you will have a local action set up which will heal a wound—will set up a healthy granulating surface, and completely heal it up in a short time, when every other

method has failed to do so. What is the condition and character of a chronic ulcer? A chronic ulcer is an excavation, for the most part affecting the limbs of old people, generally the inner side of the lower limbs; two processes are going on, the formation of the excavation, and the formation of a ring of organized lymph which encircles the excavation, thus hollowing it to a considerable depth, and its surface is covered with unhealthy lymph. This is common to all of them; the elevation of the margins is not a specific manifestation, as it is common to other forms of ulcer. Under the microscope there is not a granulation to be seen, there is an ichorous discharge, with an offensive factor arising from it, and yet there is no sensibility, no annoyance nor suffering to the patient, it goes on for year after year without any change; you may strap it, there is no change produced. I give opium, what does it do? It seems to start the capillary system all over the body, particularly in the lower extremities; it produces a genial glow, which may be compared to that of a cold bath, but not from such as is ordinarily taken; go into a warm bath, with a meal in the stomach, with or without perspiration, it makes no difference; well, then, the glow is produced by the opium, and healthy granulations over the whole surface of the ulcer will become apparent in a week. What is the sum of the actions of opium? You will say it is unhealthy. It is not; it promotes warmth, the pouring out of healthy secretions, it produces healthy integument, healthy granulations, it absorbs the unsound around it, and while the granulations are coming up the elevations are going down. That cannot be considered unhealthy which promotes health.—*Med. Press.*

NEW METHOD OF INTRODUCING MEDICINES INTO THE SYSTEM, MORE ESPECIALLY APPLICABLE TO PAINFUL LOCAL NERVOUS AFFECTIONS.—Dr. Alexander Wood has been led to introduce solutions of morphia and Batley's sedative solution into the cellular tissue, as near as possible to the affected nerve, by means of the small perforating syringe, constructed by Mr. Ferguson of Giltspur street, for injecting aneurisms with perchloride of iron. Dr. Wood narrated nine cases in which he had employed this method of treatment, in all with perfect safety, in some with complete, in others with partial success. As to the *modus operandi* of this method of treatment, he endeavored to show, from the experiments of Muller and others, that the effect of the local application of opium to a nerve was to destroy its sensibility at the part, and that from this action of the drug the immediate cessation of the pain arose. He then pointed out the rapidity with which absorption appeared to take place from the cellular tissue, which seemed to account for the rapidity of the narcotic effect which a small dose of opium so introduced was found to produce. He also pointed out, that other medicines might be introduced in the same way.

Dr. W. T. Gairdner mentioned, that a patient in his wards in the hospital, had been injected the other day in the way recommended by Dr. Wood. The result was not decisive, as the complaint for which the man was under treatment, viz: lumbago, had been undergoing rapid amendment, and, indeed, the day after the operation, was nearly gone. The experiment, however, was attended with little suffering, and it was noted that some degree of giddiness was almost immediately produced.—*Monthly Jour. Med. Science.*

NEW BROTH FOR THE SICK.—By Justus Liebig. To prepare this broth, half a pound of the flesh of a recently killed animal (beef or the flesh of a fowl) is chopped fine, and well mixed with a pound and an eighth of distilled water, to which four drops of pure muriatic acid, and from half to a drachm of common salt, have been added. After an hour the whole is thrown on a common hair sieve, and the fluid allowed to run off without pressure. The first portion, which is turbid, is poured back, until the fluid runs off quite clear. On to the fleshy residue in the sieve half a pound of distilled water is thrown in small portions. In this way a pound of fluid (cold extract of meat) is obtained, of a red color, and an agreeable taste of broth. The sick are allowed to drink a cupful cold at pleasure. It must not be heated, as it then becomes turbid, and deposits a thick coagulum of animal albumen and hæmatin.

The sickness of a young female servant from typhus, in my house, gave occasion to this preparation. It was called forth by a remark of my medical attendant, that, in certain conditions of this disease, the greatest difficulty which presented itself to the physician lies in an imperfect digestion—a consequence of the condition of the intestines, and the difficulty of obtaining food suitable for digestion and the formation of blood. Generally, broth prepared by boiling is deficient in all those ingredients of meat which are necessary for the formation of the albumen of the blood, and the yolk of egg which is added is very poor in this substance, as it contains in the whole  $82\frac{1}{2}$  per cent. of water, and  $17\frac{1}{2}$  per cent. of egg-albumen, or a substance analogous to it, and whether this substance, in its nutritive qualities is equal to the albumen of flesh is, according to the investigations of Magendie, at least doubtful. Besides the flesh albumen, the new broth contains a certain quantity of hematin, and therein a large quantity of iron necessary for formation of blood corpuscles, and, lastly, the muriatic acid for its digestion. A great hindrance to the employment of this broth in summer is its changeableness in hot weather. It undergoes fermentation, as sugar with yeast, without giving a disagreeable odor. What substance causes this change it is very desirable to ascertain. On that account the flesh must be treated with very cool water in a cool place. Ice water and cooling with ice removes this difficulty. But, above all things, care must be observed that the flesh is used fresh, and not several days old. In the hospitals of Munich and in private practice this broth has been employed with great advantage.—*Annalen der Chemie and Annals of Pharmacy*.

CASE OF ARRESTED PULMONARY TUBERCULOSIS.—By Dr. Stokes. Some years ago I saw a gentleman, who came to town laboring under all the symptoms of well-marked phthisis. The disease had been of some months' standing, and the patient was a perfect picture of consumption. He had a rapid pulse, hectic, sweating, purulent expectoration, and all the usual *physical signs* of tubercular deposit, and of a cavity under the right clavicle. I may also state, that the history of the disease was in accordance, in all particulars, with this opinion. I saw this patient in conversation with a gentleman of the highest station in the profession, and we both agreed that there was nothing to be done. This opinion was communicated to the patient's friends, and he was advised to return to the country. In about eighteen months afterwards a tall and healthy-looking man, weighing at least twelve stone, entered my study, with a very comical expression of countenance: "You don't know me, doctor," he said; I apologized, pleading an inaptitude that belongs to me for recollecting faces. "I am," he said, "the person whom you and Dr. — sent home to die last year. I am quite well, and I thought I would come and show myself to you." I examined him with great interest, and found every sign of disease had disappeared, except that there was a slight flattening under the clavicle. "Tell me," said I, "what you have been doing?" "Oh!" he replied, "I found out from the mistress what your opinion was, and I thought as I was to die I might as well enjoy myself while I lasted, and so I just went back to my old ways." "What was your system of living?" said I. "Nothing particular," he said; "I just took what was going." "Did you take wine?" "Not a drop," he replied, "but I had my glass of punch, as usual." "Did you ever take more than one tumbler?" "Indeed, I often did." "How many? three or four?" "Aye, and more than that, I seldom went to bed under seven!" "What was your exercise?" "Shooting," he said, "every day that I could go out." "And what kind of shooting?" "Oh, I would not give a farthing for any shooting but one!" "What is that?" "Duck shooting." "But you must have often wetted your feet?" "I was not very particular about the feet," said he, "for I had to stand up to my hips in the Shannon for four or five hours of a winter's day, following the birds!"

So, gentlemen, this patient spent his day standing in the river, and went to bed after drinking seven tumblers of punch every night; and if ever a man recovered from phthisis he had done so when I saw him on that occasion. Suppose, now, that he had been confined to an equable temperature, and a regulated diet, and had been treated in all respects *secundum artem*, what would have been the result?



Any of you can answer the question. In point of fact, this very treatment had been adopted during the first three months of his illness, and his recovery may be fairly attributed, not so much to the duck shooting and whiffey-punch, but to the tonic and unoppressing treatment which he adopted for himself, and which his system so much required to enable him to throw off the disease.—*Med. Times and Gazette*.

**NATURE AND TREATMENT OF DIABETES MELLITUS.**—Dr. F. Headland read, February 10, last, before the Medical Society of London, a paper on this subject. After a slight general sketch of the symptoms, he proceeded to the inquiry, What is the physiological cause of this abnormal and excessive secretion of glucose, or grape sugar? Three chief theories had been brought forward to account for this.

1. *Theory of Renal Disorder.*—By Dr. M. Good and others of his time it was supposed that the glucose was formed by the kidney in the act of secretion. The author discussed the various alleged morbid conditions of the kidney in diabetes, none of which are known to be constant. The recovery of sugar in the blood and other secretions of diabetes is sufficient to overthrow this theory.

2. *Theory of Starch-iron Assimilation:* held by Bouchardat in France, and by the majority of physicians in England. It supposes that the formation of glucose is due to a deranged digestion or assimilation. Most consider that it is formed in the stomach; others blame the liver. This notion, also, the author disclaimed—arguing that, after a meal on starchy matters, grape sugar may be found in the blood of a healthy man; that it is a part of the function of the liver to form sugar and fat out of albuminous compounds; and that this explanation does not account for the excretion of sugar, for grape sugar given to a healthy man does not pass out in the urine.

3. *Theory of Starch-iron Assimilation.*—Supported by Miesche, Liebig, B. Jones and others. To this the author gave his own assent. It derives confirmations from the experiments of Lehmann, Dumas, and D. Thomson. The starch of the food is the chief supporter of the respiratory process. Starch cannot be absorbed without being first changed into dextrine. This is a sort of transition into grape sugar, into which it is all formed in the blood. This grape sugar is not yet in condition to be oxidized; it is therefore again changed into two atoms of lactic acid (or some very similar material). This then combines with oxygen in the blood, supporting the animal heat by its combustion, and forming carbonic acid. These changes require certain agents, probably ferments, to effect them. Supposing they have proceeded as far as glucose, and the agency wanting which should change them into lactic acid—then the glucose, not being available to the system, is excreted in the urine. The liver attempts to supply the want by forming glucose and fat out of albuminous food. This glucose passes also into the urine. In addition to all this waste, the very tissues are preyed upon to supply fuel for the respiration. The author then discussed at length the subject of treatment, under the following heads:

**A. Erroneous plans of Treatment.**

1. Attempts to prevent the formation of glucose.
  2. Attempts to hinder the excretive function of the kidney.
- The first is a natural process; the second is a healthy provision.

**B. Doubtful plans of Treatment.**

1. The use of diuretics.
2. Stimulation of the nervous centres, as by strychnia.
3. Treatment directed to the liver.
4. The use of oxidizing agents.

**C. Correct plans of Treatment.**

*Dietetic Rule.*—To supply, if possible, such articles of food as shall be able, at the same time, to nourish the patient, and to maintain the respiratory combustion without passing through the stage of glucose. (Among other things, fat and oils, dry wines and milk, were recommended.)

**Therapeutic Indications—**

1. To give some remedy that shall seem to be capable of causing the glucose to



undergo its normal transformations. Yeast, rennet, pepsin, etc., were discussed. The author particularly recommended milk just turned sour, as containing a decomposing casein, which transmutes milk sugar into lactic acid. He had advised the use of this remedy in his *Essay on the Action of Medicines*. It should be used as an article of diet; or it may also be given in enemata, and in warm footbaths.

2. To replace the urinary secretions by means of diaphoretics and purges. (The author recommended copious sweating, for a physiological reason.)

3. To attend to the general health, and to treat complications.—*Medical Times and Gazette*.

**BORAX INJECTIONS IN INFANTILE DIARRHŒA.**—M. Bouchet considers infantile diarrhœa to be of two kinds: the one, symptomatic of lesions of the intestinal mucous membrane; the other, idiopathic, a nervous or catarrhal flux from the great intestine, which may occasion death, without leaving any material morbid appearances. In the latter variety, M. Bouchet recommends clysters containing the bi-borate of soda, as peculiarly efficacious. This remedy proves as beneficial as it does in aphthæ of the mouth, it acts as a weak astringent on the intestinal mucous membrane, and as an alkali in neutralizing the acid secretions poured out by it, which leads secondarily to ulceration of the bowels, and especially of the anus.

He uses clysters containing from 10 to 20 grammes of the salt, in 125 grammes of water.

In cases of infantile diarrhœa symptomatic of intestinal ulcerations, M. Bouchet has found benefit from the use of borax, administered internally in the dose of two grammes, in eighty grammes of a mucilaginous emulsion.—*Gazette des Hôpitaux*.

**TRIAL OF PHOSPHORUS IN PARAPLEGIA.**—Prof. Bennett has given phosphorus in seven cases of paraplegia from disease of the spinal cord, in the form of phosphuretted oil (4 gr. of phosphorus dissolved in 3j of olive oil.) In none of these cases has he been able to satisfy himself that any improvement was occasioned. He commenced with three drops a day, which were afterwards cautiously increased to ten, and in one case to fifteen drops. But these large doses soon induced violent nausea and vomiting, and, after a short suspension of the remedy, he continued it in doses of three drops.

In the case of chronic myelitis, which took ten and fifteen drops, the phosphorus was excreted by the lungs, as the breath smelt strongly of the drug, but was not phosphorescent at night—a phenomenon which has been seen by some physicians who have employed it. In another case, that of a delicate woman, with probable congestion of the cord, a large amount of phosphate was passed in the urine presenting, under the microscope, beautiful feathery crystals. The dose of the oil, therapeutically, ought never to exceed five drops, and even this amount cannot be administered for any length of time without deranging the stomach.—*Monthly Jour. Med. Sci.*

**THERAPEUTIC ACTION OF LUPULIN.**—Debout's observation on the favorable influence of lupulin in quieting painful erections induced Dr. Zambuco to institute some experiments with the medicine, which, to prevent mistakes, he administered to the patients with his own hands. The form was threefold: pure lupulin, the tincture, or mixed with an equal part of sugar. The doses were different: of pure lupulin, 1—16 scruples (these high doses caused no disturbance of the nervous system;) the tincture, 2—16; the mixture with sugar, 1—2. From eight well-marked cases of painful erection, consequent on gonorrhœa, out of a much larger number, the author was enabled to draw the following conclusions: Lupulin possesses a remarkable effect upon the genital organs; the erethisms were quieted in four-fifths of the cases. It is of therapeutic utility when there exists the necessity for keeping the penis in a state of rest. Camphor given in large doses, with a similar object, both irritates the alimentary canal, and often fails in its end. The sedative and anti-blennorrhagic influence of lupulin depends upon a resinous,

æthero-oily principle; while the bitter principle yields a real tonic. The author has seen lupulin given with good effect in scrofula.—*Med. Times and Gazette*, from *Schmidt's Jahrbuch*.

DIAGNOSIS OF TYPHUS AND TYPHOID FEVERS.—By Dr. Parkes. You are brought to see this young woman, we will say, for the first time; the specific rose-spots are gone; she is laboring to all intents and purposes under severe bronchitic and chest symptoms (a chemist, or practitioner with a druggist's shop, has prescribed, and given cough mixtures, perhaps, without seeing her;) you find her respiration thirty in a minute, cough incessant, with some expectoration, nervous symptoms also well-marked; vertigo complained of, torpor, the eyes closed; she is delirious at night; she has also diarrhœa, pain over the abdomen, pulse quick, tongue furrowed and somewhat coated. Suppose, I say, you were called to such a patient, and moreover she is unable to give any account of the previous illness, how are you to make the diagnosis? There are only two ways—one the positive method, the other the method as it is called by "exclusion." The first is obvious enough, and will of course be more valuable to the practiced eye of the experienced physician, who seizes the nature of the case at the first glance by a sort of intuitive knowledge of what typhoid really is. Now the method of diagnosis by exclusion, the plan of logic-writers, *per viam exclusionis*, in this and other diseases, is one, though not without advantage, one yet of no mean importance. The first question you resolve in your mind will be—is she or he, as the case may be, laboring under any of the idiopathic fevers? any of the exanthemata? No. Is it typhus? You make the same answer, as the eruption in ty-phus is as different from ty-phoid as scarlatina from measles. The eruption is absent in patients under 22 or 21 (this patient's age is about this.) Is it relapsing fever, so common some years, as 1828-29? No. You ask yourself, then, is it typhoid? Yes. Nervous symptoms are marked, chest symptoms and diarrhœa, also; the latter loose, granular, yellow, so peculiar to typhoid. You have soreness of the right iliac fossa; but then you say we have no rose-spots, and then you remember in at least 20 per cent. these rose-spots are not found. You must weigh and balance all these circumstances in your mind.—*Med. Times and Gaz.*

FRACTURE OF THE HEAD OF THE ASTRAGALUS.—M. Vollmar relates (*Zeitsch. f. Chirurg. und Geburtsh.*, iii., 1854,) the following example of this rare accident:

A stout countryman fell from the height of eight or nine feet. After the removal of the boot from the left foot, there was noticed a bony prominence in the arch of the foot; and the patient, imagining there must be a fracture, applied cold lotions to the part to reduce the swelling. The foot was found in its natural position, with moderate flexion. In front of the articular extremities of the tibia and fibula there lay, under the raised integument, a bony swelling, separated by a deep depression from the outer malleolus. No hollow, from which the bony mass had escaped, could be detected; the tarsal bones seemed in their normal relations. Extension effected the reposition of the displaced parts, and in four weeks the patient was able to go about, proper bandages having been maintained to keep the separated portions of the astragalus together.—*Med. Times and Gaz.*

TREATMENT OF HOSPITAL PHAGEDÆNA.—The prevalence of phagedæna, which, during the past nine months, has been pretty general in the London hospitals, seems now to be steadily diminishing. It has been very difficult to arrive at any satisfactory conclusions respecting the laws regulating its occurrence. At one period, it has appeared to spread through a certain ward, or to prevail in a certain hospital, as if by contagion; while at others, observations have been made tending strongly to support the opinion, that it was largely under the influence of atmospheric changes. It has prevailed very irregularly in the different hospitals, being now epidemic in one, and then, after the lapse of a short time, appearing in another. On the whole, it has been a mild form of the disease. Very few have, we believe, died of it, and a vast majority have recovered after a short and not very destructive attack. In several instances, however, in which stumps, after amputation, have been attacked by it, so much of the soft parts have been de-

stroyed, that a second removal of the bones has become necessary. With regard to the treatment of the disease, the following recommendations might, we think, be summed up as the results of the combined experience of the surgeons who have been most engaged with it:

1. *As soon as a wound shows a tendency to become sloughy or phagedænic, to have the patient change his bed, and if possible, his ward.*—This practice was pursued in almost all cases in Guy's and in the London Hospital, and more or less in most others. Often very sudden benefit was remarked. The recommendation, of course, proceeds on the supposed desirability of removing the patient away from any local influences, contagious or endemic, which may have had part in producing the disease. The following case may be quoted as illustrative: A boy in excellent health submitted to primary amputation of the arm, on account of a crush, under the care of Mr. Wordsworth, in the London Hospital. On the day following the operation he was remarkably well, and had not the least constitutional disturbance. During the next six days he continued well, and the stump was granulating healthily, when it became necessary to change his bed, and to put him into one from which a man who had died of pyæmia had been removed. Mr. Wordsworth directed that all the bed furniture should, as a measure of precaution, be removed; and, with the exception of the mattress, this order was complied with. On the morning following the change of bed, the lad was feverish and restless, and his stump had lost its granulations and presented a sloughy surface. He was at once ordered back to his original bed. The phagedæna did not spread; but almost immediately after the second change, the condition of the stump began to improve, and ever afterwards the advance was uninterrupted.

2. *To destroy fetor by the employment of charcoal.*—In this way, probably, not only is a gas decomposed which was likely to have acted prejudicially on the animal functions, but one which might not improbably have been the means of direct infection.

3. *To employ nitric acid as an application to the sore.*—Most surgeons have formed a very high opinion of the value of this remedy when efficiently used. The acid should be concentrated and pure, and should be liberally applied. We have already at such length spoken in its praise, that anything further need scarcely be here added. (See *Medical Times and Gazette* for January 6, page 9.)

4. *To employ as constitutional measures tonics and general stimulants, with, in some cases opium, or the chlorate of potash as specific remedies.*—Respecting the use of the latter, a considerable difference of opinion prevails; but instances have occurred in some hospitals which appeared to show almost incontestably their potency in at least some individual cases. The chlorate of potash well deserves a much more thorough investigation as to its remedial powers than it has yet received.—*Medical Times and Gazette.*

TREATMENT OF ULCERS BY ANAPLASTY.—Having in our preceding No. (pp. 276-78) noticed the rival claims of Dr. F. H. Hamilton and Dr. John Watson, to a method of treatment of ulcers by Anaplasty, it is but justice to the former surgeon to state that in a subsequent reclamation (*New York Jour. Med.*, Jan. 1855) he denies that Dr. Watson has not anticipated him in either the proposal or the results; but states that Dr. Watson has misapprehended the principle of his operation. This operation consists in transplanting a piece of new and perfectly healthy skin upon the centre of the ulcer, by which means he hopes not only to supply an amount of skin equal to the size of the piece transferred, but to furnish, also, a nucleus from which additional skin shall be formed—to establish a new centre of life—an oasis, from whose outer verge a true and healthy vegetation shall advance in every direction over the exhausted soil.

"It is not improbable," he remarks, "also, that the graft will itself expand, or be drawn centrifugally by the contraction of the surrounding granulations and cicatrix, conversely, as the skin about the ulcer had before been stretched and drawn centripetally, by a similar action of the granulation and cicatrix situated within its free margin, so that, after a time, it will cover more space, independent of any actual growth, than it did originally. The opposite of this happens usually in anaplasty, and would occur here, did the flap equal or exceed in size the



wants of the parts to be supplied. The flap would contract, thicken, and project itself above the surface. But in old ulcers, it will generally be found impossible to furnish a direct supply of integument equal to the loss. A deficiency must probably still exist, and sufficient it is believed, to determine in the transplanted skin a necessity of expansion."

He further states that his proposal dates from the year 1844.

**CHLOROFORM AND ETHER COMBINED AS AN ANÆSTHETIC.**—M. Robert, in his report on chloroform to the Société de Chirurgie, recommended a mixture of equal parts of chloroform and ether as the best anæsthetic agent. M. Cellaries has subsequently published (*Gaz. Méd. de Montpellier*, 1853,) the results of experiments made by him with the compound upon rabbits, etc.; and he reports very favorably as to its efficiency as an anæsthetic. The two fluids combine together, without the occurrence of any precipitate, and the odor of the compound is said to be far from disagreeable.—*Gaz. Méd. de Paris*, No. 96, 1854.

[This combination was first employed, we believe in this country, and is preferred to either article alone by several of our practitioners.]

**ABSENCE OF CHLORIDES FROM THE URINE, DIAGNOSTIC OF THE ONWARD PROGRESS OF PNEUMONIA.**—In the April number of the Monthly Journal of Medical Sciences, Prof. Bennett gave an account of certain facts which confirmed the statements of Simon, Redtenbacher, Beale, and others, as to the absence of chlorides in the urine during the onward progress of pneumonia. The professor resumed these inquiries on returning to the clinical wards of the Edinburgh Hospital last summer, and in the number of the above mentioned Journal for December last, he relates three cases of the disease there treated which confirm the diagnostic value of this change in the urine. In all these cases the absence of chlorides marked precisely the onward march of the pneumonia, while their presence indicated its cessation and was generally accompanied by the returning crepitation and commencing absorption of the exudation.

**NEW REMEDY FOR PRURITUS VULVÆ.**—Dr. Scholz recommends an Indian plant, the *Caladium seguinum*, which is used by the natives of India as an anaphrodisiac, for the treatment of those cases of this most distressing malady, which are due rather to a hyperæsthetic condition of the parts, than to any lesions of the mucous membrane of the vulva. The plant belongs to the *Aroideæ*. Scholz has used it with great success in two cases which had previously resisted all remedies; and he administered it in the form of an alcoholic tincture in doses of six drops.—*Arch. Gén. de Méd.*

**ANAPHRODISIAC PROPERTIES OF BROMIDE OF POTASSIUM.**—Thielmann recommends this remedy as an excellent anaphrodisiac in satyriasis, in the frequent and painful erections during gonorrhœa, in spermatorrhœa, and in nymphomania. He administers it in doses of from 2 to 3 grains every two or three hours; and, at the same time, enjoins a mixed vegetable and milk diet, and forbids all acid substances.—*Med. Zeit. Russl.*

**SULPHATE OF QUININE IN PHTHISIS.**—D. Muntendam, a Dutch physician, has just published a paper to show that, from his experience in twenty-two cases, sulphate of quinine, given along with acetate of morphia, or even alone, is capable of prolonging the life of the patient in many cases of phthisis, and that it may even effect a cure in those cases in which a tubercular deposition has just commenced, especially in married women and children. He alleges that, when given continuously in small doses, it does not cause dyspnœa, diarrhœa, headache, or any disagreeable effects; and he believes that in very many, but not in all cases of phthisis, it should be ranked as one of the best remedies for the disease.—*Month. Jour. Med. Sci. from Nederlandsch Lancet.*



## Editorial and Miscellaneous.

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As our journal is supposed to be the organ through which we express the sentiments of that large and increasing class known and designated by the title of "Young Physic," we beg leave to present again a few thoughts on the subject of *specialities* in practical medicine, and the advantages likely to accrue from such a systematic division of study and labor as has for years past been adopted by European practitioners, both in medicine and surgery, and is now fast coming into vogue among our Northern brethren. In this matter we believe there can be effected a wholesome improvement upon the system which has now the sanction of time-honored usage, but which we conceive has no other merit to commend it to our acceptance. In attempting the discussion of such a topic, we have no wish or design to criticise the existing mode of practice, or to denounce it on account of its defects, but rather to indicate some tangible and practical method of improvement and reform. We venerate all those institutions which for a long lapse of time have ministered to the good of mankind, but we deprecate the blind devotion of those who adhere to a system simply because it is old. *Conservatism* is a principle worthy of all honor, and should govern us in all matters of vital importance, but there is a principle paramount to it, and worthy of still greater honor—and that is, a manly and enterprising *progressiveness*.

One reason why we advocate such an innovation in the practice of medicine by the new generation now preparing for active service in the profession, is found in the fact, that the science of medicine has advanced so rapidly, and embraces so wide a field at the present day, that it is questionable whether any one is able to acquire a knowledge of all its varied details in the time—the longest time which can be allotted to the purpose. Each branch of medical study and research has, within the last half century so extended its domain, that to gain a knowledge of one, now requires as much time and labor as the achievement of the whole would have demanded fifty years ago. It is said that the progress made in certain sciences is so rapid, that it would require the constant reading and study of one man, in order to keep himself "posted," as the phrase is, in the experiments and discoveries made in such individual science. This is alleged to be true of geology and chemistry, and we doubt not that the field of pathology and physiology is extending itself in the same ratio. Not content with the study of these sciences as nature presents the subject to the unassisted vision, microscopic analysis has been pressed into the service, and thus the subject has been magnified a thousand

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fold. A thorough acquaintance with all the various branches which enter into the preparatory course of a practitioner of medicine is absolutely impracticable in the time commonly devoted to this purpose, and the consequence is, our schools must remain satisfied with sending forth men superficially, rather than thoroughly educated for their important vocation. In this is probably to be found the chief cause of the prevailing ignorance and corresponding assumption of a large number of the medical profession at the present day. Too much is expected of the student. Each year adds to his task, with no equivalent in time, and of necessity the country is flooded by a half-learned generation of medical men; as though the attempt were made to supply the defect in quality by the annual increase in quantity, and gain in number that which is lost in weight. Meanwhile, we deplore the degradation of the profession, its loss of social standing, and the triumph of charlatanism, without once stopping to inquire whence comes this state of things, and what causes are in operation to produce it.

We believe a wise and systematic division of the practical labors of physicians in this city, would tend very much to remedy some existing evils. So far as the country practitioner is concerned, we cannot see that any change is practicable. His education must be extended and complete. Like Minerva springing from the front of Jove, he must emerge from his Alma-Mater, armed at every point. In surgery, he must be possessed of certain attainments, or he will often be pained by seeing some fellow-mortal lose life or limb through his ignorance or mal-practice. He must be well skilled in obstetrics, for he is likely to meet with cases which would have taxed the abilities of a Denman or a Dewees. In his own right hand are his only resources as an oculist, and on his own trained ear must he depend to interpret all those signs which a diseased organism makes audible. He must also comprehend to a considerable extent the sciences of chemistry and toxicology, or in some sudden exigency of medical jurisprudence, he will, like Degberry, be compelled to write himself down an ass. But in this city no such necessity obtains, and here it is that a systematic division and arrangement of the different branches of practical medicine will be found to enure both to the benefit of the profession and the welfare of the public.

New Orleans is now and must ever continue to be the medical metropolis of a larger extent of country than any other city in the Union. Four States are dependent on it alone for all those advantages which are naturally sought in large cities, while from a much more extended region invalids are constantly finding their way hither in search of medical aid and relief. Here, then, should be found those who, having made the various departments of scientific medicine the subject of special and thorough investigation, are therefore prepared to give these sufferers from abroad the full benefit of their judgment and skill. And this we consider the more urgently called for, since it is a truth capable of the fullest demonstration, that no man is able to acquire a complete knowledge, both practical and theoretical, of the entire science of medicine, unless, indeed, he be favored with extraordinary facilities, and such as are by no means afforded in the ordinary curriculum of the schools.

There is another view of this subject which gives it additional importance, and to which it will be well that we direct attention at this time. It has come to pass that a vast horde of quacks, charlatans and other advertising wretches have established themselves in this city, many of them permanently, and a goodly number who make periodical visits. All of them, however, being bold beyond measure in their profession, and well skilled in the art of newspaper puffing, they profit not a little in their unprincipled traffic in the lives and health of the unwary. For the greater encouragement of this species of imposition, our Legislature, not long since, granted it the support of a legal charter by repealing the license laws which had previously been enforced against empirical practitioners of the healing art. Upon what plea of justice or humanity this action was founded, we have never been able to discover. We venture to assert, that no such enactment, touching the legal profession, would have found Legislative favor and endorsement. That profession still continues to receive the protection and guardianship which proper restrictions and requirements are calculated to afford. Nor can we be convinced, that the medical profession is not entitled to the same privilege and protection, until it can first be shown, that property is more precious than life, or that the conservative influence of the one profession is less promotive of the public good than the penal processes of the other.

But we have no intention to argue this point. We simply advert to the fact which must be palpable to all, that the *laws of Louisiana do not protect the public health*, or afford any security against the unlimited spread of imposture and empiricism in the practice of medicine. To remedy as far as possible these growing evils, it becomes us to enter the lists against these pretenders, and endeavor to cope with them by turning their weapons upon themselves. It is a favorite method with such men to present themselves to public notice, as possessed of more than ordinary ability in the management of special diseases, and attract the public eye by associating their name and fame with a single class of the long catalogue of human maladies. In European cities, the benefits arising from such a distinction are appropriated by the scientific practitioner, and we can see no good reason why the same system cannot be established with us. It cannot be supposed that the public, however blind to its own interest in these matters, will voluntarily make choice of an empiric in preference to one thoroughly skilled in his art, and having the imprimatur of a scientific school in his favor. Let them at least have an equal position and stand upon the same platform, and if an enlightened and discerning public fail to appreciate the worth of a scientific education, and entrust life and health in the hands of quacks, let them do it, and reap the consequences.

We offer these crude and desultory views as our feeble plea in behalf of *specialities*. Whether the practice of them is to be established among us or not, we leave the profession to decide. However this may be, we rejoice in the assurance, that our noble science has not yet reached its maximum. It still manifests signs of *vitality*, and must ever combine in exact proportion the elements of *progress*. It must either advance or decline, for should the day ever come when it shall cease to grow, it will at the same time begin to perish.

"THE YOUNG PROGRESSIVES."—

"Even the divine science of Medicine, as much as it may shock the nerves of the *Young Progressives* to learn the fact, has not escaped the immutable laws of nature, which, so far from commanding progress to our science, compels us, like herself, to revolve perpetually in a circle, and like herself, so sure as spring, autumn and winter succeed each other and present new aspects, but always arrive at the same point of departure; so does medical science, notwithstanding it may exhibit new forms, fashions and phases, remain the same, moving forever in a circle and never making the slightest progress."

"In what particular has the world progressed for centuries past?"

"In what branch of medical science have any improvements been made for a century, aye, for eighteen centuries?"

"Modern physiology is nothing but a tissue of speculation and conjectural abstractions, and he who possesses the most wild and visionary fancies is the best physiologist. It is all hypothetical."

"What progress has pharmacy made? it is as it has always been the mere derivative of accident or empiricism."

"Chemistry, what useful progress has chemistry made for centuries past? None."

"And as to chloroform — if a fraction of the atrocious disclosures, said to have been made by the New York dentists, in the case of Dr. Beale be true, it were well if all the chloroform in the universe were collected and made a bonfire of, and its administration pronounced a felony, by legislative enactment, throughout the world."— *Thoughts on the Young Medical Progressives of New Orleans, the Hospital Gazette, etc. etc.* By one of the "Old Uns."—*True Delta*, April 18th, 1855.

The above stuff is in answer (?) to the leading editorial of our last number — an editorial which the "Old Un" does us the honor to admit, was written in a good temper.

In telling us that medical science has made no advances, the "Old Un" errs in this; that he gives us his own private personal experience as the records and conclusions of history. Although this is the common vice of historians, we must protest against it.

There is no necessity for an answer to the "Old Un." The "thoughts" of every reader of this article overthrow each assertion as he reads it. But even if his own article were not, by its preposterous assertions, a sufficient answer to itself, we conceive it beneath professional dignity to enter into a medical controversy with an unknown writer in a daily newspaper. Medical controversies should be carried on in medical publications. The quackish art of entering into newspaper controversies has been discovered and made sufficient use of by the sarsaparilla men. We do not think that either the "Old Un" or we can improve upon them, and we are determined never to be second best in any art we undertake.

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NEW METHOD OF DISSIPATING THE RANCID ODOR OF FATTY SUBSTANCES.—M. Griseler discovered accidentally that the addition of a small quantity of nitric ether to oils, has the effect of entirely removing any rancid odor which they may possess. Evaporation by heat to drive off the alcohol of the ether, leaves the oils as limpid and sweet as ever.

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Professor Langenbeck, of Berlin, makes use of chloroform as an injection in hydrocele, he much prefers it to the tincture of iodine now so much in vogue; he attributes the cures effected by this substance to the alcohol of the tincture and not to the iodine it contains. He injects from one to three drachms into the sac.



Dr. E. Aleix, in an article addressed to the *Moniteur des Hopitaux*, speaks highly of the uses of Colodion in the treatment of varicose veins. The pressure produced on the part, after evaporation has taken place, is equal and well kept up, and as a palliative remedy, it can not be too highly praised. In variocoele he applies it to the scrotum, and the effect is such as to render it difficult to discover by the touch, the bundle of veins.

M. Aleix has also used the Colodion in contusions with extravasations of blood, hygroma, and synovial tumors, with marked success.

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M. Bonnet, of Lyons, in a late article in the *Journal de Medecine et de Chirurgie Prat.*, speaks most favorably of the treatment of lumbago by *kneading* (*Massage*); the operation is performed by the thumb and fore-finger, the part being first anointed with simple cerate to prevent excoriation.

M. Nelaton has recently had an opportunity of testing the treatment, and he reports most favorable results. In one case, that of a patient who was suffering the most intense pain, after the kneading had been kept up for ten or fifteen minutes, great relief ensued, and although the pain was not entirely removed, it was quite bearable.

Before resorting to kneading, M. Nelaton applies chloroform to the part by means of compresses, covered with oil silk to prevent evaporation, and in many cases of lumbago and rheumatic torticollis this has sufficed to afford relief.

The application of chloroform and kneading to the treatment of muscular rheumatism may be, therefore, looked upon as an improvement in the treatment of an affection, the duration of which, if left to itself, is rarely less than from six to ten days.

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M. LeCoeur, Professor at Caen, recommends friction with strong vinegar in itch. He reports ten cases of cure—no failures.

The vinegar is applied, three times daily, by means of a sponge; friction being sufficiently strong to rupture the vesicles. The average duration of the treatment was five days.

Mr. LeCoeur also recommends vinegar as a preventive and cure for the bites of those very annoying insects, known to us as the *red bug*.

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TANNIN IN ALBUMINURIA.—M. Van Holsbeck, of Brussels, has used tannin, in large doses, in the treatment of one case of this disease, and with marked success; the dose of the remedy was gradually increased, until the quantity taken per day amounted to  $\text{ʒi.}$ , the treatment was continued about a month.

M. Van Holsbeck was induced to use it on account of its well-known hemostatic action and its property of decreasing the force of the contractions of the heart; from these facts he was led to believe that the passage of the albumen of the blood through the kidneys might be prevented by it. The results in this case go to prove the correctness of his hypothesis.

M. Jules Guerin has lately read a memoir to the Académie de Médecine on the operation of paracentesis thoracis, by what he calls the sub cutaneous method. The instruments he uses consists in a flat trocar curved at its extremity, and a pump, each furnished with a stop cock, the latter so arranged as to admit of the aspiration and discharge of the fluid without displacement of the instrument. The operation is performed by pinching up a fold of the skin at the point where the opening is to be made; the trocar is then introduced in such a way that the thoracic and cutaneous punctures are about an inch apart. The operation, when thus performed, obviates the danger of the entrance of air from without, and also the exposure of the thoracic wound which is covered by the slipping back of the integument to its former position.

Of fifteen patients operated on in this manner at the Hôtel Dieu, Val de Grace, and Dépôt de Saint Denis, upon whom the operation was performed thirty times, eleven recovered entirely, and five died. Of these latter, one had an organic disease of the heart, three were effected with pulmonary tubercles, and one had traumatic pleurisy.

Such results as these speak favorably for an operation, which is usually looked upon as most formidable, and rarely had recourse to, except as a *dernier resort*.

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CHARITY HOSPITAL REPORT—Month of April, 1855:

Admitted, - - - - -	709
Discharged, - - - - -	691
Died, - - - - -	91
Births, 15—8 males, and 5 females—2 still-born.	

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Our Journal has now such a list of subscribers, as to make it a valuable medium of advertising to Druggists, Surgical Instrument makers, Bandagists, and others, whose occupations are either remotely or directly connected with the profession. We therefore place before them the terms of the New Orleans Medical News and Hospital Gazette, and solicit their patronage.

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NEW ORLEANS

MEDICAL NEWS AND HOSPITAL GAZETTE.

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NO. IV.

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Remarks on Yellow Fever.

By JOHN HARRISON, M. D.,

*Professor of Physiology and Pathology in the Medical College of Louisiana.*

[Concluded from April Number.]

PROGNOSIS.

Some of the symptoms I have already spoken of as being unfavorable; but it may be worth while to sum up their value in a general way.

A case of yellow fever promises to terminate well or ill, in proportion to the developement of the fever, and according to the absence or presence of nervous symptoms. When the rigor is slight; when the ensuing fever is well developed; when the pulse is open, full and strong, beating from 108 to 120; the eyes a little injected; the tongue slightly furred; when the pains in the head, back, and limbs are severe, the case is likely to do well, provided it be properly treated and nursed. On the other hand where there is a strong disposition to sleep; where there is not much complaint heard; where there is either sullenness, listlessness, and extreme languor, or agitation of the mind from fear of death, or any other cause; where the pulse is weak and quick, or easily changed in its beats by muscular motion, such as sitting up in bed; or, where the skin is pale, sallow, or cool to the touch, the case is one of extreme danger. In the above remarks, I refer to symptoms occurring in the beginning of an attack; during the progress of the disease, other symptoms make their appearance; these will be spoken of presently. All marked changes in the usual expression of the countenance, either at the commencement, or occurring as the disease progresses, are decidedly unfavorable symptoms. Contraction of the brows, risus sardonius, twitchings about the mouth, picking of the bed-clothes, are of this character. It is another unfavorable sign to find

the patient listlessly lying on the side of the bed with his head drooping over. Petechiæ are also unfavorable symptoms, particularly if they make their appearance as early as the third day. Great and unaccountable depression of spirits, either in the beginning, or in the course of the attack, is an exceedingly bad symptom. Delirium, coming on late in the attack, is a fatal symptom; so, also, is suppression of urine, which must not be confounded with retention. Sighing and moaning are bad symptoms. Irritability of the stomach, coming on after the febrile excitement has subsided, is another bad symptom; in the beginning, however, I do not consider it of much consequence, if the other symptoms be favorable. Neither do I so consider jactitation, when it occurs early in the attack, and is caused by pain; jactitation without pain is another thing, and a very bad symptom. When the tongue continues to look clean and healthy for a day or two; or when it becomes clammy, after the fever has subsided, the prognosis is unfavorable. Passive hæmorrhages, and black vomit have been already spoken of. With regard to the sweats so common during the febrile stage of the attack, they are thought by many to be favorable. I do not think them of much consequence, one way or the other; certainly, I have lost patients, in spite of every attention and precaution, who perished finely during the first days.

I believe I have now spoken of the most important of the symptoms, with the exception of the strange and exquisite tenderness, which comes on late in the disease, at the epigastrium, and indeed, over all the body. It is the most fatal symptom I know of.

#### TREATMENT.

Of all the diseases which afflict the human race, there is none that requires more unremitting care and attention on the part of both physician and nurse, than yellow fever. Accidents, or acts of imprudence, which, in other diseases, are mere trifles, are of tremendous importance in this. the mere getting out of bed has cost many a man his life. Exposure to cold currents of air, or negligence to take the requisite precautions against a change in the weather, has been equally fatal. A man in this disease, however safe the physician may think him, is hovering between life and death—a trifle may decide his fate. Hence the great necessity, the all-important need of good and experienced nurses.

There is, from the very commencement of the attack, a great and rapidly increasing prostration of strength, inconceivable to those who have never



experienced the disease. The mind cannot act; the senses, at first exceedingly acute, become, during the progress of the disease, indifferent to impressions; the muscular power is almost annihilated; the patient is indifferent to fate, or morbidly anxious about trifles. The extraordinary disturbance in the nervous system, its extreme liability to undergo change from the slightest impressions, enforce upon the attendants and physician, the greatest prudence and solicitude. Experience has taught *them* this, but, unfortunately, all injunctions are frequently lost upon the sufferers. *They* cannot be made to understand how the mere getting out of bed, or even sitting up in it, can be of so much importance as they are asserted to be. Their feelings deceive them; they make in some unguarded moment the trial, and conviction comes too late.

When a person is taken sick with this disease, no time is to be lost—not a minute. The physician and nurse should be with him as early as possible. His room should be in some quiet place, and, if possible, in the second story, on account of the dampness of the ground floors. The windows should be closed, for many suffer much from intolerance of light. The room should be well aired, care being taken to protect the patient from currents. All persons, except those attending on the sick, should be rigidly excluded; conversation on the part of the patient, or others, prohibited. These last injunctions are all-important, for it is not an uncommon thing for the sick to be annoyed with unseasonable visitors; who, to gratify an idle curiosity, rush in where they can do no possible good, and may inflict measureless harm. The patient should not, from the very hour of his attack be permitted to rise from his bed, for any purpose whatever. No matter how supported, or with what precautions and care he be taken out, it is always dangerous, and often leads to a fatal result. His evacuations should be received in a bed-pan, and removed immediately from the room. There ought, if practicable, to be two or more nurses, so that the patient should never be left alone for a single instant. His bed-clothes and person should be kept as clean as possible; but all changes of linen, etc., without absolute necessity, deferred until the patient is out of danger.

As to the medical treatment, the like precautions are necessary. The physician should never forget, for an instant, the peculiar character of the disease—its treacherous nature—the rapidity with which alarming symptoms come on. He must bear in mind that the patient is hourly losing strength; that his nervous system is becoming more and more deranged. He must remember that there is no safety for his patient until the disease

has run its course, and convalescence established beyond all doubt. Those who see the disease for the first time, are exceedingly apt to make a serious mistake: the fever subsides on the third or fourth day—the pulse and skin are good—the patient complains of no pain, and the physician supposes him out of danger. The truth is the danger is then most imminent—the most critical period of the disease has arrived, and the patient is required to be watched more assiduously than ever. It is at this stage that a purgative, or any other medicine improperly administered, may decide his fate.

As to the details of the treatment, they must be left to the judgment of the physician. Any specific treatment is just as absurd in yellow fever as in any other disease. The physician is not called in to treat an abstraction, but a sick man. Treatment must be varied, according to the peculiarities of the cases. Remedies, beneficial in one case, may be most injurious in another; and success in practice will depend, in a great degree, upon the sagacity and acquirements of the physician.

Certain modes of practice, however, have prevailed here, as elsewhere. All have had their advocates, who point to results as evidences of their value. If we were to rely upon statements of partizans, it would be difficult, indeed, to form an opinion of their respective merits; but it must be remembered, that patients in yellow fever die, and that others get well, under all sorts of treatment. I was once called to an Irishman, who had been sick five days, and who had done nothing but drink whisky the whole time. He was suffering with great irritability of the stomach, but recovered. It is not, then, from such statements that we can form any correct opinion concerning the *methodus medendi* in this disease. We must fall back upon the broad principles of pathology and therapeutics.

Of the methods in vogue, we may point out three that have had the largest number of advocates. They may be denominated the Calomel, the Depleting and the Quinine practice. I shall proceed to make some comments upon each of them.

*Calomel.* I should perhaps speak of this practice in the past tense; at least I know of no physician in New Orleans who pursues it, or mentions it with respect. Absurd as it is, however, it has had as strenuous supporters as any other delusion in medicine. It has not been more than twelve years, when he was a bold man who undertook to affirm, that a case of yellow fever might get well without the aid of calomel. The practice seems originally to have been adopted in sheer desperation, and continued from the same cause.

The treatment consists in bringing the patient, as soon as possible, under the influence of the drug. For this purpose, ten or twelve grains are administered every two or three hours. Should time pass on, however, and the patient show no symptoms of salivation, the dose is either increased, or the intervals between its administration lessened. Should we ask for some pathological reason for such practice, we are told, that the calomel acts by emulging the liver; that experience has pointed out the necessity of the treatment; and that every case is saved in which pytalism occurs. Let us examine these arguments.

As for the liver, the symptoms of the disease throughout its whole course, as well as *post mortem* examinations, show that it is by no means particularly affected. The passage of bilious stools, during the first days of yellow fever, is as common an occurrence as we meet with, though not a grain of any mercurial has been taken. We find, also, bile in the gall-bladder after death; so that the whole argument about the liver is just upset by these facts.

It may be contended, however, that the yellow hue of the skin, etc., is caused by the accumulation of the principles of bile in the blood; and that, therefore, the liver should be excited to increased action in order to eliminate these principles from the system. I do not believe that the yellow hue of the skin is owing to bile; we frequently see the skin, at first of a bright pink, gradually assume the yellow tinge, as if it depended upon some change in the coloring matter of the blood. But, for the sake of argument, let us grant that the bile is the cause of it; does the administration of calomel prevent, or even retard its appearance in the slightest degree? Not at all. The yellow skin—the passive hæmorrhages, etc., are just as bad, to say nothing more, in cases treated with mercurials, as in those in which not a grain has been given.

But, whatever effect calomel may have upon the liver, it is very plain that administered as it usually is, it must first act upon an organ just as important to the welfare of the system, and which, *post mortem* examinations show, is by far the most frequently affected. I mean the stomach. The connections of this organ with the rest of the system are so numerous, that some have even called it the centre of the sympathies; its extreme irritability is one of the most marked traits of the disease; its serious derangement is what the physician most particularly dreads; and can any one believe, that we shall shun this danger by administering a mineral drug, such as calomel, every hour or two?

As for experience proving the necessity of administering calomel, I shall dismiss the subject with the remark, that experience has proved just the contrary.

But all cases recover, in which mercury produces ptyalism! Admitting this to be true, which is by no means the case, it is but reasoning *post hoc ergo propter hoc*. The disease runs its course—the fever subsides—the patient recovers *in spite* of the remedy, and the poison introduced into the system then takes effect. Instead of a rapid convalescence and a speedy restoration to health, as is usually the case in yellow fever, he is the martyr of a most noisome and insufferable disease, for weeks or even months; and is fortunate, indeed, if he gets off so well.

In 1833, there occurred a phenomenon which was as common as any other in the disease. It was suppuration and ulceration of the parotid glands. It is now rarely met with, and the reason is, less calomel is given.

The parotids were not the only glands that suffered. In the autopsies of that year, it was a common thing to find the mesenteric glands swollen and enlarged, as to meet with any other lesion.

By the foregoing remarks, I do not wish to be understood, as inculcating the total abandonment of calomel in yellow fever. Given as a cathartic, in the commencement of an attack, I have seen it act admirably. It causes but little nausea, and will bring away feculent matter, when castor oil, or saline purgatives, fail to do so.

*Depletion.* All the characters of this disease would seem to inculcate in the strongest possible manner, the greatest reserve and discretion in the use of the lancet. Of this instrument, all powerful for good or for evil, according to the mind that directs it, it has been long ago remarked, that, perhaps, its victims numbered more than those of the sword. Sure, I am, it has fully done its work in yellow fever.

On the first days of the fever before the patient is much prostrated; when the pulse is full and strong; when the pains are severe; when the patient is of robust constitution, venesection unquestionably does good. Whenever during the febrile period, it is to be feared that congestion is forming in any organ—the brain, lungs or stomach—the lancet should be employed, but employed with prudence. The physician should never forget—that this is a disease arising from poison—that prostration is rapidly approaching, and that by improperly using the lancet, he is hastening its advent and adding to its intensity. When the patient is of a nervous temperament, or feeble constitution—when any ataxic symptoms



supervene, such as nervous delirium, etc., the lancet should not be thought of. Some have used this instrument, as if they thought it possible to bleed the disease out of the body. A greater error was never committed. Large quantities of blood are taken from the patient, who has been made to sit up, and syncope supervenes. In the course of an hour or so, observe his pulse. Has his fever abated? are his sufferings less severe? Not a whit. His skin is as hot, his pulse is as bounding as ever, but has lost its force. Standing some distance from the patient, we can see the carotids violently throbbing. We feel the pulse, and it has a peculiar thrill. Again and again is the lancet employed; and more and more grave all the symptoms become. The patient begins to wander in his thoughts, and speaks incoherently. "The brain is becoming inflamed," says the physician "and he must lose more blood;" again the lancet fulfils its office, and a change sudden and appalling, takes place. The patient becomes cold and pale—a clammy sweat breaks out over the body—the pulse sinks—black vomit is thrown up in large quantities, and death soon follows. This is no fancy sketch—it is what may be witnessed every epidemic year.

The nervous system is particularly liable to derangement in yellow fever; the most fatal of all the symptoms are those which indicate affections of the brain. Now, there is nothing that has a more powerful control over the nervous system than the lancet, and hence, extreme caution is necessary in its use.

But for what purpose is so much blood taken from the system? What indication is to be fulfilled? Is it to relieve organs suffering from inflammation? *Post mortem* examinations ought to settle the question. In the worst cases, those of a congestive character, there are scarcely any lesions to be found after death; and as for the congestions usually found in the stomach and intestinal canal, it is far more probable that they are of the same nature with the petechiæ, and other congestions, which we see form on the surface of the body, than that they are the results of inflammatory action.

We are told by Andral, that in idiopathic fevers there is a diminution of fibrine in the blood. In yellow fever the blood is remarkably slow in coagulating, and when passive hæmorrhages occur it will hardly coagulate at all. Assuredly we cannot expect to increase the proportions of fibrine in such a disease by blood-letting.

With regard to the use of other means of depletion, such as leeches and cups, the same prudence and caution are required. As they take blood,

however, in a slower and more gradual manner than the lancet, there is not the same risk of affecting the nervous system. They are, therefore, much safer. Cups are frequently very serviceable in relieving the intense pains in the loins and head, which the patients suffer during the febrile period.

*Sulph. Quinine.* This remedy had often been employed as a tonic during the latter days of an attack, but in 1839 a new mode of administering it was adopted at the Charity Hospital, and in the private practice of many physicians. This mode, so far as I know, was first put in practice by Dr. J. M. Mackie, of this city, who adopted it at the suggestion of Dr. Thomas Hunt, also of this city. Dr. Hunt was led to believe in its efficacy from a review of the writings of Maillot, published in the July number, 1839, of the British and Foreign Medical Review. From that work I make a few extracts:

"The first of these publications consists of a memoir read to the Royal Academy of Medicine, founded upon observations made or collected by the author, in the garrison of Bona, relative to the destructive epidemics of the years from 1832 to 1835. In those years, the garrison, consisting of between three and four thousand men, 22,530 were admitted into the hospital, and 2,513 died, or 1 in 8; according to the more particular statement, there were

"Admitted in 1832,	.....4,038,	of whom 449 died, or 1 in 7
"          1833,	.....6,704,	of whom 1526 died, or 1 in 3½
"          1834, }	.....11,593,	of whom 538 died, or 1 in 26
"          1835, }		

M. Maillot's attention was first and principally directed to determining the analogies existing between the fevers of Bona and those which he had previously had an opportunity of observing at Algiers and in Corsica, with a view to deciding on the propriety of applying to them the treatment which he had found adapted to the latter. The establishment of the characteristic of intermittence, as common to both, seems to have been the result; the fevers of Bona, like those of Corsica and of Algiers, arising in the neighborhood of marshes, the greater proximity of which to the troops at Bona, gave a severer character to the fever; demanding, as it proved a prompt and more energetic treatment. M. Maillot maintains the relation of the continued forms to the intermittent; and the tendency of the intermittent, if unchecked, to pass into the continued; and of the continued, if bleeding was employed, to pass into the intermittent or remittent. These circumstances, he says, convinced him that he had not to deal with true continued fevers, the gastro-enteritis or gastro-cephalitis of France. He concluded that the probability was that the affections before him were those spoken of by Torti, part of the character of which is "*de intermitte sensim, acutam et malignam migrat*;" and resolved on giving the quinine boldly in all the continued cases, without waiting either for remis-

sions or intermissions, which were 'only instantaneous when they were obtained.' The results which are very striking, are seen in the diminished mortality exhibited in the above statement."

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"In the Bona fever, M. Maillot says it was possible, as late as the first days of June, to cause a kind of remission of the symptoms. This remission, however, appears to have been no more than the morning remission of symptoms, so common in the continued fevers of our own climate; and even this was only induced after blood-letting; by which sometimes the fever was entirely subdued, and sometimes converted into a distinct intermittent; it often seems, on the other hand, to have run on to malignant and typhoid forms. And, at the end of June, the continued fevers were quite distinctly separated from the intermittent. Yet the continued fevers, it would seem, begin occasionally with a few intermittent paroxysms; after which they pursue their course even without remissions, however slight. The point of practice which M. Maillot is most anxious to enforce is, that, notwithstanding this appearance of continuity, the treatment demanded was the administration of bark in full doses. The same circumstances, and the necessity for this practice, were pointed out by M. Coutanceau in the epidemic pernicious fevers of Bordeaux, in 1805; and his opinions are quoted by M. Maillot, who declares them to be equally applicable to the fevers of Bona of 1832 and 1833. With these convictions, M. Maillot gave large doses of sulphate of quinine in all cases of continued fever, with the exception of some in which there was ileo-colitis, in which, although he thinks he was wrong in making them an exception, he deferred its administration. In all the cases thus treated, in which he enumerates cases of gastro-cephalitis, of acute gastro-enteritis, of follicular ileo-colitis (diarrhœa), of hæmorrhagic ileo-colitis (dysentery), etc., the disease, except in a few instances, was relieved in a few days. In almost all these cases, the patients began to take some light food on the third or fourth day. Of ninety-eight cases of gastro-cephalitis included among them, occurring in the month of July, not one became typhoid; and only five died, of whom two sunk the day after admission into the hospital. In the other cases, the solution of the disease was speedy, and the convalescence rapid.

There is something surprising in this account, and we have allowed the reader to share in our astonishment, although, at page 26 of his memoir, when M. Maillot comes to relate particular cases, we find another article of treatment generally preceding the use of the sulphate of quinine, and which, although it is no other than pretty free bleeding, general and local, had not been before as of the smallest importance.

"A soldier of the 59th, aged twenty-five years, was admitted into the hospital on the 8th of August, on the second day of an acute and excessively intense gastro-cephalitis. I immediately prescribed bleeding from the arm to fifteen ounces, the application of forty leeches to the epigastrium, and twenty leeches in the course of the jugulars; low diet; lemonade.

"On the 8th, at the morning visit, the reaction was not entirely subdued; but the condition of the pulse, that of the skin, and all other symp-

toms, denoted a remission indicative of approaching remittance or intermittence; and I consider it a continued gastro-cephalitis, passing into intermittent or remittent fever. Low diet; lemonade; twenty-four grains of sulphate of quinine, to be taken in a potion at one dose, and immediately.

“Complete apyrexia established itself during the day. The apyrexia continued on the morning of the 10th: nevertheless, I prescribed another potion of twenty-four grains of sulphate of quinine, fearing that the fever might be tertian, and return the next morning. But the fever did not return; and convalescence went on rapidly. On the 18th, the patient was nearly on full diet.” (p. 27)

“Such was very nearly the treatment of 295 cases of gastro-cephalitis; except that the sulphate of quinine, in subsequent cases, was given immediately after the venesection; and, in certain circumstances, before any sanguine evacuation, as many of the men had been carried off by paroxysms of pernicious fever, some hours after the opening of a vein. Of the 295 cases thus treated, only twelve died or 1 in 24. These results were certainly satisfactory: but M. Maillot observes, that such treatment would not be suitable to cases occurring in the north of France, in which dangerous typhoid affections, and (in case of recovery) tedious convalescence, would be the consequences. There can be little doubt that such would be the serious results of similar practice in the continued fevers of England; yet we believe there is much evidence of the most respectable kind among previous writers on the fevers of the Mediterranean, and of Italy, and of Africa, in support of the practice observed by M. Maillot.

“Among the cases of true intermittent fever, 1582 were quotidian, 730 tertian, and 26 quartan. Of these 2338 cases of intermittent fever, the accession took place between midnight and noon in 1652, and between noon and midnight in 686. The greatest number of accessions took place between nine in the morning and noon. 658 of the cases were simple and 1680 complicated. 1078 instances the intestinal canal was affected; alone in 343 cases; with the brain in 686 cases; with the lungs in 31 cases; with the brain and lungs in 13 cases. In 25 cases the spleen alone was diseased; and in one case the peritoneum alone. The brain was affected alone in 466 cases, the spinal cord in one; the lungs alone in 103 cases, and the pleura alone in five. In one case, a tertian, there was angina with the formation of a false membrane, and no other lesion. The intensity of all the complications was in direct ratio to the elevation of the temperature; and they were always unfavorably affected by the wind of the desert.”

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“In this country, pathologists have not been very ready to admit this theory of intermittent inflammatory conditions of the intestinal canal and brain. M. Maillot presents the doctrine in the least objectionable form. The secondary congestions, he says, are ordinarily very feeble in the first accessions, and disperse in the interval between one accession and the next. Complete apyrexia therefore ensues, without functional disorder of the digestive or respiratory passages. But when the accessions are many times repeated, and, above all, when they assume a quotidian type, each leaves some anatomical traces of congestion in the viscera affected. The capillaries soon become unable to disembarass themselves of the blood which



each accession determines to them; the tissues soon become unable to resist a state of congestion so frequently renewed, and the irritation "fixes itself anatomically," and betrays itself by symptoms more or less continued. Hence arises a prolongation of the reaction; that is to say, of febrile symptoms of a gastro-enteritis, a gastro-cephalitis, a pneumonia, etc., according to the organs which are over-irritated (*surirrités*.)

"To these remarks, M. Maillot adds the very important practical observation, that simple irritations, and those not of great intensity, and which yield in the intervals of an intermittent, give rise to symptoms in this class of fevers as marked and violent as those of acute gastro-cephalitis. This circumstance, he observes, if unknown or unattended to, might lead the practitioner to see inflammations where none exist, and to be afraid of administering the quinine, on which alone the hope of preventing the returning accession of congestion must rest. In illustration of his practice, M. Maillot inserts a case in which, after bleeding during the paroxysm to fifteen ounces, the patient presenting the symptoms of acute gastro-cephalitis, twenty-four grains of sulphate of quinine were given at once, thirty leeches were applied to the epigastrium, and there was not another paroxysm. Of 250 cases thus treated, he lost only 11, or 1 in 22; the fatal cases were all quotidian. In the pernicious forms of fever, with coma, he gave forty grains of the sulphate of quinine at a dose; and in one such case, 148 grains were given in less than twenty hours, and the patient, from being in a state of coma, almost resembling death, became speedily and completely convalescent. In cases of the *algide* form, or with extreme coldness, ether was administered with the sulphate of quinine

"The most convincing proofs of the correctness of the above practice, and perhaps of the theory also, is that M. Maillot appears to have reduced the mortality in the fearful epidemic he had to contend with from 1 in 3½ to 1 in 20; for these results cannot be ascribed to any alteration in the character of the disorder; but became sensible when he began to use the sulphate of quinine more freely than he had ventured to do at first, and to bleed less copiously. Subsequent engorgements of the abdominal viscera, dropsy, diarrhœa, so often considered to arise from the use of bark, were scarcely seen in any case; and M. Maillot considers them as the results, not of the medicine, but of repeated paroxysms of the disease."

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"Of the different kinds of bark, M. Maillot prefers the red (*oblongifolia*), not only to the gray (*lanceifolia*), which contains only cinchona and not quina, but also to the yellow (*cordifolia*), from which, on account of the quina it contains, the disulphate of the London pharmacopœia is prepared. But he prefers the sulphate of quinine (disulphate of quina) to all preparations of bark; the doses being more easily regulated, the stomach tolerating it better, and its action being surer and speedier; qualities which practitioners are not all equally inclined, we think, to accord it the possession of. When the stomach rejected it, the sulphate was given in a lavement; if purging or cholæc ensued, the endermic method was had recourse to; and sometimes the patients were put into a bath saturated with cinchona. In salicine and ilicine, M. Maillot places, as may be expected, little confidence. He seems not to have given any trials to opium;

but he quotes the opinions of Lind and of M. Bailly, to show that its power is only sedative, and not febrifuge; and that, if it seems to shorten the duration of the attacks, it does not prevent their recurrence. We have little doubt that there are cases in which opium mitigates the sufferings incidental to intermittents; and in some cases, at least in our own climate, a large dose given in the cold stage, will put an end to it, and even to the disease altogether. M. Maillot agrees with M. Bailly in condemning the combination of antimony with opium as useless. With less reason he utterly condemns arsenical preparations. He also reproaches the notion of giving an emetic or even a purgative before commencing the administration of the sulphate of quinine; being of opinion that the attention of the practitioner, being directed to the subduing of abdominal or other irritations by leeches, would be uselessly given to removing any internal sources of irritation, which he looks upon as imaginary, and but the relics of exploded doctrines. In this respect, theory certainly interferes unfavorably with M. Maillot's practice; and that there are even cases of intermittent in which, after the application of leeches to the epigastrium, and the administration of a purgative, the disorder will disappear before a grain of bark is given, every one who has seen much of ague must have found. The general propriety of M. Maillot's rule however, not to delay giving the sulphate of quinine as soon as a complete apyrexia is established, cannot be controverted; and the old doctrine of coction and crisis led, without doubt, to hurtful delay, during which the constitution suffered greatly from repeated paroxysms. He is, as we have seen, decidedly an advocate for giving the sulphate in doses of twenty or more grains, which he administers in four ounces of water; and the time he prefers is three or four hours before the expected attack. His experience in Africa determined him at length not to repeat the medicine more than once or twice after the suppression of a paroxysm; and he has pursued the same plan with success since his return to France. The prolonged use of the medicine is objectionable; and he agrees with M. Hepple in believing that it does not even prevent the return of the fever, after a given time, so certainly as having recourse to it anew, at the expected periods of return; supposing such periods to be ascertained, as stated by M. Hepple, namely, the eleventh and twenty-first day in quotidians and tertians, and between the twentieth and thirtieth in quartans.

"The unfortunate tendency to a relapse makes it desirable, M. Maillot observes, that the patients should consider themselves as convalescent for two or three months, however well they may appear to be. When relapses take place, bleeding, except by leeches, is generally less required than at first; but the sulphate of quinine should be given in increased doses. In some cases a complete change of residence is indispensable to a recovery; soldiers who were sent home to France after obstinate and renewed attacks often recovered health during the voyage."

The new mode of administering the sulphate of quinine may easily be inferred from the foregoing extracts. It consisted in waiting for no abatement of the fever, but in promptly giving the sulph. quinine as soon as possible after the attack. The dose varied from 20 to 80 grains, given in a little cold water. If the stomach was very irritable, it was given by

injection. When the practice was first introduced a cathartic was generally first given, and the administration of the quinine deferred until the bowels were moved. This practice was afterwards abandoned, on account of the loss of valuable time. In strong and robust constitutions, the lancet was employed for the purpose of producing a temporary remission, during which the quinine was given. This practice was decidedly beneficial. If the first dose failed in eight or ten hours to produce an apyrexia, a second was given. The earlier it was given the better, but *it should not be given after the second day*. This is a most important point in the treatment; I have given it on the third and fourth day but always with injury to the patient. It increases the irritability of the stomach, and causes the tongue to become dry. In addition to the sulph. of quinine, other remedies were used according to the circumstances of the case: such as would suggest themselves to any physician.

Before venturing to give the sulphate of quinine in such large doses and upon such a raging fever, Drs. Hunt and Mackie performed a number of experiments to test its physiological action. The experiments were performed in 1839, at the Charity Hospital, upon convalescents from different diseases. From notes furnished me by Dr. Hunt, I transcribe the following cases:

CASE 1.—At 20 minutes of 10 A. M., pulse 72, took 15 grains of sulph. quinine. At 11 A. M., pulse 60; has slept; complains of heat at epigastrium; feels a little dizzy; pupils slightly dilated. At half past 12 P. M., no other change.

CASE 2.—At 10 A. M., pulse 56, took 11 grains. At 11 A. M., pulse 48; has slept; has slight ringing in the ears; sweating freely a short time after taking the quinine. No restlessness.

CASE 3.—At fifteen minutes of 10 A. M., pulse 64; took 9 grains; at 11 A. M., pulse 52; has slept; pupils slightly dilated; no other change.

CASE 4.—At fifteen minutes of 10 A. M., pulse 100, took 20 grains. At 11 A. M., pulse 96; has slept; pupils slightly dilated; no other change. At fifteen minutes past 11 A. M., took 15 grains. At half past 12 P. M., pulse 92.

All these cases were watched. The effects, as narrated, gradually wore off, without any other change.

It may be mentioned here, that when the sulph. quinine was given in large doses, a part of it was found, in the experiments made for that purpose, unaltered in the urine.



So much for the method of administering the sulph. of quinine ; it now remains to speak of its effects. The fever in most cases was cut short as if by enchantment. I shall never forget the surprise I felt, the first time I witnessed its effects. Three patients in the wards of Dr. Mackie were put under its influence. The conditions of the three were pretty much the same—as also were the results ; the description of one case will therefore suffice for the whole. The patient a robust young man of about twenty-eight years, had been taken with yellow fever at 1 P. M. He was prescribed for the same day, at 6 P. M. Ten cups were ordered to the epigastrium, 30 grains of sulph. quinine to be taken by the mouth, immediately after the cupping, and 40 grains by injection. He had been cupped before I saw him, but had not yet taken the quinine. His condition was as follows : pulse 120, full and strong ; great heat of the skin ; great pains in the head, back and lower extremities ; tongue a little furred ; eyes heavy and a little injected ; great restlessness on account of the pains. I saw him next morning, between 6 and 7 o'clock. He was perfectly free from pain ; the pulse was at 84 ; skin cool ; in short, every vestige of disease had disappeared. From prudential motives, he was kept in the hospital four or five days, and then discharged. There was never any return of the disease.

The other cases terminated in a similar manner, and the practice was soon adopted by a number of physicians, myself among the number. The results were, in general, highly satisfactory.

I shall say but few words, with regard to the manner in which quinine acts, to produce such effects. No doctrine of stimulants, counter-stimulants, or sedatives, can reach the explanation. Nothing that we know of, can be substituted for quinine ; its effects are, therefore, specific, and are owing to its chemical character. I believe that it acts, like the poison of which it seems to be the antidote, directly upon the nervous substance—breaking up the whole condition upon which the morbid actions depend, but in a manner, perfectly inexplicable in the present state of science. If there be any way of cutting short an idiopathic fever, it is assuredly to be effected by quinine. But, there are certain facts which lead me to believe, that the disease under consideration runs its course, even after all febrile symptoms, whatever, have disappeared. In 1841, I lost a patient, and in 1842, another ; one on the 6th, the other on the 7th day, in whom the fever had been cut short, as usual, by the administration of quinine. They lay in bed for two or three days, without a single symptom of disease, and, indeed, were kept in bed more from motives of prudence, on account of a change in

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the weather, than for any other reason. Yet they both died with black vomit.

I have remarked in another place, that the pathology of yellow fever was, in part, to be inferred from some points in its treatment. I alluded to the effects of quinine in the first stages of the disease. Surely, these effects give no support to the supposition that the disease arises from local inflammation.

From the above observations, concerning the effects of quinine, it will not, I hope, be understood, that I advocate its administration in all cases whatever. As I have before remarked, there can be no specific treatment for yellow fever, or any other disease. When in the commencement there is great congestion of blood in the brain, or any other important organ; or, where the fever supervenes upon chronic inflammatory diseases, I would most certainly resort to other means. Nor would I be understood as speaking of quinine as an infallible remedy. The practitioner to whom the disease is a new one, will soon discover that in certain cases, particularly those of the congestive and ataxic types, that quinine is as inefficacious as any other remedy. In cases, in which the fever is well and fully developed, it will unquestionably, cut the fever short, and thus prevent the formation of those local congestions which are produced by the febrile action. In this consists its value, and assuredly it is a great one.

It remains to say a few words concerning other remedies sometimes employed in the course of treatment. We shall first speak of baths.

*Cold Bath.*—I have but little experience in the use of cold baths, as I have been averse to employing them from pathological principles. There is a great tendency to sudden changes in this disease, and the congestive state is one of the most fatal forms in which it can present itself. When the fever is high and fully developed, the danger is far less. Even so unfavorable, in my opinion, are any symptoms of congestion, that I always look upon it as a very bad sign, when the patient, after the second hour, complains of being chilly and hot at the same time. Now cold baths, in many cases tend to prolong this congestion; or to produce a chill when the fever was about being developed; and in other cases, in which its administration is followed by quick reaction, the fever would have been sufficiently high without it. At any rate, it should only be used in the first days of the disease, for the obvious reasons, that it fatigues the patient to be often taken out of the bed; and in latter stage, it is inadmissible, on account of the prostration of the patient.

*Warm Bath.*—Warm baths in congestive cases may be serviceable in

promoting reaction; but when the fever runs high, and particularly if it be accompanied by ataxic symptoms, it does no good, but harm — the skin soon becomes hot and dry — the eyes and face flushed — and the pulse more bounding than before. In the latter days of the attack, its employment is improper for obvious reasons.

*Cold Affusions.*—I have used cold affusions in some violent congestive cases, with the hopes of producing, by the sudden shock, a reaction, and full developement of the fever; but uniformly without success. The patient when put to bed and enveloped in blankets, had but slight reaction, and that of an ataxic character.

*Sponging and Foot Baths* —By far the best mode of applying water, is by sponging the surface of the body, and by the frequent administration of hot mustard foot baths. The first should be applied to the head, throat, chest and upper extremities — the temperature to be regulated by the condition of the patient. If his fever be burning hot, and he complains of heat, he will bear even ice-water. Should he complain that the sponging produces chilliness, the water should be tepid or quite warm — a little vinegar mixed with the water, promotes its evaporation. Ice applied to the head also gives great relief to the patient, but should it produce chilliness it is subject to the same objection as sponging with water.

The sponging keeps the skin moist — relieves to a great degree the sufferings of the patient, and moderates the burning heat of the surface. The foot baths tend to equalize the circulation, and to relieve the insufferable pains in the legs. The skin should never be permitted to become dry during the febrile action — and the foot baths should be frequently repeated.

*Emetics.*—In persons attacked immediately after meals, I have employed an emetic of pulv. ipecac., in order to relieve the stomach promptly of its contents; but have used them in no other way. In this way, I have seen it do no harm. Dr. Cartwright, of Natchez, has employed tart. antim., in doses from three to ten grains, given every one, two, or three hours, dissolved in a little water, or what he considers better in the form of pills. It is only to be given in the first stage of the disease, and in cases of congestive or ataxic character. "Tartar emetic," says Dr. C., "used in this state, restored sensibility to the torpid organs, produced secretion, and destroyed the ataxic character of the disease, by establishing a general and equable excitement; or, in other words, converted an irregular and intractable condition of the system, into an open, plain and manageable case of

fever." I have no experience in regard to this treatment, but think it well worthy the attention of the profession. The great objection to using tart. antim., is, it tends to increase the irritability of the stomach, so prominent a feature in this disease. But the supposition that inflammation exists in the stomach from the commencement, is a mere bugbear; and if, by the administration of any remedy, we can break up those fatal forms of congestion and ataxia we sometimes meet with, and produce an open, well developed fever, we should do so, even at the expense of increasing the irritability of the stomach. When the fever is fully formed, we can manage it — otherwise, not.

*Purgatives*.—The bowels should be evacuated as early as possible, and thoroughly. They, moreover, should never be permitted to remain unmoved over 24 hours; in other words, their own proper secretions should be removed. The best purgatives are of the milder kind; blue pill, or a little calomel, followed in a few hours by a dose of castor oil, and some mild saline laxative. Drastic cathartics do injury by irritating the intestinal mucous membrane. Costiveness is not a very common symptom in this disease, and, after the first evacuations, clysters are generally sufficient to keep the bowels open. The repetition of purgatives, after the bowels are well emptied, do no good, but often a great deal of harm. They seem to dispose the intestinal canal to sanguine engorgements, and consequently, to passive hæmorrhages.

*Narcotics* —Opium and the salts of morphia are sometimes administered to check the incessant vomiting which supervenes in the beginning of the last stage; sometimes, also, they have been given to relieve the hiccough, and sometimes to quiet the patient in nervous delirium. In neither case have I ever seen good effects from their use. Without effecting the object aimed at, they appear to check at once the already too greatly diminished secretions.

*Blisters, etc.*—Applied to the epigastrium, blisters are sometimes of great service in relieving the gastric irritability in the commencement of the last stage. Their administration, however, requires caution, for if they are too large, or administered in improper cases, they do mischief by affecting the already exhausted nervous system.

*Stimulants*—Such as brandy, ale, port wine, carb. ammon., camphor, etc., are often resorted to, towards the close of the attack, but generally in cases altogether hopeless. In many instances, however, I have thought that they did a great deal of good—the patients recovering under their

use. The extreme prostration, to which the patients are generally reduced after the febrile stage is gone by, demands some artificial support to be given to the system. The early administration of a little English ale, or sangaree of port wine, I have often thought has turned the scale of life in favor of the patient. But it must be remembered that yellow fever is not typhus fever, and that the like benefit from stimulants cannot be expected.

The remaining classes of the *Materia Medica*, such as tonics, diaphoretics, diuretics, etc., require no particular notice.

I have now concluded the task I undertook. In a future number I may offer some speculations, concerning the etiology of this disease.

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## Adversaria Medica.

*By I. L. CRAWCOUR, M. D., Visiting Physician, Charity Hospital.*

At a late meeting of the Louisiana State Medical Association, it was remarked by the President, Dr. Stone, that if physicians, instead of occasionally meeting for the mere purpose of disputing on some obscure point in physiology, or splitting hairs on mere doctrinal differences, would come together for the purpose of severally stating the experiences each had had, what had been seen and what done, what errors had been committed and what fallacies avoided, much good would accrue to the profession at large, as each would gain by the experience of his professional brethren. I think no one can avoid coinciding with the views thus taken by one who may be considered the Nestor of Medicine in this city, and great would be the advantage, were his advice generally followed.

It is perhaps impossible that frequent meetings of the great body of the profession can take place, but it is certainly within the power of every man to contribute his mite to the stock of general knowledge. In medicine, no two cases are exactly alike; they may be analogous, but not parallel, and the great advantage of the publication of cases is, that men are enabled to draw comparisons between what they see and what they read—in many instances to the advantage of themselves and their patients. Systematic works on medicine and monographs of particular diseases are undoubtedly valuable; but books of clinical instruction, such as Andral's *Clinique*, Lee's *Cases in Midwifery*, and Grave's *Clinical Medicine*, are



invaluable, and are the works especially adapted to the practitioner. In short, as Mr. Micawber would observe, what we want are not so much views as facts; and in connection with this subject, I cannot help transcribing a most apposite and striking passage from Southey's Doctor:

"A ship captain," the Doctor used to say, "when he approaches the coast of France, from the Bay of Biscay, or draws near the mouth of the British Channel, sends down the lead into the sea, and from the appearance of the sand which adheres to its tallowed bottom, he is enabled to find on the chart where he is, with sufficient precision for directing his course. Think, (he would say,) what an apparently impossible accumulation of experience there must have been before the bottom of that sea everywhere within soundings could be so accurately known as to be marked on charts, which may be relied on with perfect confidence. No formal series of experiment was ever instituted for acquiring this knowledge; and there is nothing in history which can lead us to conjecture about what time sailors first begun to trust to it. The boasted astronomy of the Hindoos and Egyptians affords a feebler apparent proof of the false antipathy of the world than might be inferred from this practice. Now, if experience in the ART OF HEALING had been treasured up with equal care, it is not too much to say, that therapeutics might have been as much advanced as navigation has been, by preserving the collective knowledge of so many generations."

After perusing these observations, I think none can doubt of the advantage of making known what one has seen, and especially in what are called common cases. It is at present too much the fashion to run after, with avidity, what are called interesting cases; that is, cases which in their very essence are incurable. Thus, a double murmur of the aortic valves, or a large cavity at the apex of the lung, will draw crowds of admirers, while the cases that occur in every day life, and which make up the great bulk of practice, and which are frequently as difficult to cure, are left unnoticed, or treated after some orthodox routine plan, whose only advantage is that it saves the practitioner the trouble of thinking.

I propose in this and the following papers, to state what I see in private as well as in public practice, and trust and expect, that any errors I make may not escape the strictures of my professional brethren. An old teacher of mine, Dr. Addison, once said that he always gained more by making a mistake than by a success, and I think his observation will be found generally true. In three of the cases I shall present in this paper,

the chief interest lies in the large quantity of iodine administered, without any of the evil effects so carefully enumerated in books. If we look in Pereira, we shall find that one-fourth of a grain of iodine is a very large dose, and that the form of pill is decidedly mischievous and injurious for its exhibition. I have given it in four and five grain doses three times daily for months without any injury following, and to the decided benefit of the patient.

**CASE I—*Sacculated Bladder.***—Mr. H., a resident of this city, applied to me in December. He complained of a constant desire to urinate—rarely being able to retain water above an hour, and frequently the intervals being shorter. He suffered much from pains shooting across the lower part of the abdomen and fundament, and suffered much pain in the bladder when it was empty. His appearance was very anæmic—tongue red at edges—the urine normal. I introduced a No. 8 silver catheter into the bladder, without any difficulty, and drew off about two cunes of water, he having micturated about half an hour before. He complained of a sense of soreness while the instrument was passing the prostatic portion of the urethra. On examining the prostate per anum it felt hot and somewhat engorged, and pressure on it produced severe pain in the penis.

His spirits are very despondent; his complaint has lasted three years. Thinking that the irritability of the bladder might depend in part on the anæmic condition he was in, and in part on the engorgement of the prostate, I prescribed the following:

**R** Potass tartrate of iron.....gr. v.  
Hoffman's anodyne.....3 ss.

in infusion of Buchu, three times daily, and 5 grains of the iodide of potassium, night and morning.

After an interval of eight days I again saw him, and he was somewhat better; the pain in the perinæum was less, but on drawing off his water by the catheter, just at the termination of the flow of urine, I experienced a sensation as if a smart blow were given to it, and on moving the instrument a quantity of urine, amounting to about three ounces, again flowed.

Thinking this might be caused by stone, I sounded carefully for it, but found none. I then came to the conclusion that this phenomenon was due to the existence of a pouch in the bladder, and was one of those cases of fluttering stroke of the bladder depending on a cyst, described by Guthrie.

On the 15th of December I injected the bladder with warm water and threw in about half a pint without producing much uneasiness.

December 23.—Much improved; can retain urine now for three hours. Since the 18th I have injected his bladder every day with warm water and ordered

Syrupi ferri iodidi.....	gr. ss.
Potassi iodidi.....	gr. iii.
Spts. ætheris sulph.....	gr. i.
Tinct. Buchu.....	3iss.

*Ex aquâ ter in die.*

<i>R</i>	Ext. conii.....	gr. v.
	Hydrarg. massa.....	} aa gr. i.
	Pul. rhei.....	

*Nocte et mane.*

January 22.—Has continued the above medicine up to the present time. His general health has much improved. The bladder has been injected every day with warm water and he now retains, without any pain or difficulty, 12 ounces. In addition to the injection, I left in the bladder every day an ounce of water, containing muriatic acid and morphine, commencing with one drop of the former to the ounce of water, and gradually increasing the proportion of muriatic acid. To-day I injected seven minims to the ounce, which he retained.

February 19.—Passes water now only four times in the twenty-four hours. Can hold 15 ounces; feels only occasional uneasiness in the perinæum and is undisturbed at night. Ordered—

<i>R</i>	Iodinii.....	gr. vi.
	Morphiæ muriat ...	gr. ii.
	Ext. Belladonnæ .....	gr. iii.

To make 12 pills—one every three hours.

The proportion of iodine in the pill was gradually increased to four grains, three times daily; he was directed to take them after meals. They produced no disturbance, no symptoms of iodism appeared, and he gradually improved and is now well. The pouch of course has not disappeared, although from the fact of the impulse felt on the catheter after withdrawing the urine, being less, I am disposed to consider it diminished in size.

This case is interesting, first from the fact of there being a pouch in the bladder, and secondly, from the quantity of iodine given, without its producing any disturbance. The sacculated condition of the bladder is, I believe, not common, and the sensation produced is so similar to that of a calculus, that one is very liable to be deceived. I believe the first person to draw attention to this peculiar symptom was Mr. Guthrie. He describes the first case in which he detected it to have occurred in the year 1816. The patient was a soldier. On examining with the catheter, a smart blow was felt on the instrument coeval with the termination of the flow of

urine, simulating a calculus. The blow, however, was deficient in the sound which a solid, hard substance gives. In two other cases, the sensations were the same, resembling the blows given by the wings of a bird in fluttering, from which circumstance Guthrie called them fluttering strokes of the bladder. He considers them as caused by the pouches containing urine, being propelled against the instrument by the muscular efforts of the bladder in the evacuation of the last drop of urine from its cavity. It is probable, that in this case a large amount of the extreme irritability of the bladder was due to the fact of the patient never being able thoroughly to empty his bladder, a certain quantity always remaining in this pouch. The bladder had also acquired a habit of contracting on the reception of a small quantity of urine, which I endeavored to correct by the gradual introduction of warm water. This and the acidulated injection produced great relief. The iodine, independently of its constitutional effects, I believe, acted locally, as it is always found in the urine as an iodide soon after administration, and I have no doubt acts topically on the bladder as long as it is in contact with its walls. If administered *after a meal*, it can be given in the pill form, in tolerably large doses, and I have never seen any inconvenience result from its use.

[*To be continued.*]

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## Case of Lithotomy:

*Reported by ORICK METCALFE, M. D., of Natchez.*

Hamilton, aged 10 years, negro. The subject of the operation had been suffering, ever since he was a few months old, with gravel. More than a year ago he became a patient of mine. His general health was very good, notwithstanding he suffered excruciatingly at times. Occasionally the stone would close the neck of the bladder, and produce all the distress attending the too great distention of the viscus. His urethra was very small and his bladder excessively irritable. A metallic sound of any size was so firmly held by the neck, owing to its great irritability, that no perquisition could be made, and the stone could only be felt with the gum elastic bougie. His health was so good that I thought much preparation unnecessary. For two weeks before the operation, I made him drink freely of a weak solution of the bi-carbonate of soda.



On the 12th April, I proceeded to operate, assisted by Dr. John S. Scott. After the patient was put under the influence of chloroform, the grooved staff was introduced, and the stone distinctly felt. The cutting was more difficult than on the dead subject, but was completed with very little loss of blood. A stone, composed of the oxalate of lime, measuring one and one-eighth inch in its long diameter, and one inch in its short diameter, was withdrawn. The bladder was very much thickened, and the mucous membrane rough and bleeding. No opiate was administered, as there was not the least suffering. On the eighth day, the urine began to flow through the urethra. On the sixteenth day it ceased to flow by the wound, and in eight days afterwards he was walking about perfectly well.

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### Communicated.

*Editors of the New Orleans Medical News and Hospital Gazette :*

GENTLEMEN—I take pleasure in complying with your request, that I should furnish you an account of a very interesting case which has recently fallen under my observation.

May 18th, at 8, A. M., I was called to Carrollton to the infant of Mr. D., of Burdette street, in that place. I arrived at the house at 9, P. M., and found an infant boy of four months, (one of a pair of twins,) *in articulo mortis*. He was evidently dying of cerebral congestion.

I was told by the friends that he had been sick five days; had had fever, accompanied with diarrhœa; had been for the last twenty-four or thirty-six hours restless, thirsty, and often moaning in its sleep; had refused to suck, and about an hour before my arrival, had been seized with a violent convulsion, and had vomited “black stuff” pretty freely. The handkerchief on which it had vomited was produced, and on it was that which, to my mind, presented all the appearances of dried “black vomit.” On the clothes of the child was also to be seen this peculiar stain. The extremities, breast and neck of the child presented a bluish mottled appearance.

At 9½, A. M., the little patient suddenly, and without exertion, threw up at least four or five tablespoons full of that which I call pure “black vomit,” and almost immediately thereafter expired.

I distinctly understand this child to be but four months old, and a native of Carrollton. It was born within four squares of the house where it died, and was of highly genteel parentage. I am also informed by the friends, that the twin brother of the deceased, and the weaker of the two, had just passed through a similar attack of fever and diarrhœa.

I have, in order to be on the safe side, read the foregoing to the father of the deceased, and it meets with his entire approval as to correctness. I make no comment on the case, but submit it as "food for reflection," for those who are opposed to the doctrine of the origination of yellow fever inside the mouth of the Mississippi river.

Your obedient servant,

D. WARREN BRICKELL.

NEW ORLEANS, May 23, 1855.

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### Communicated.

*Messrs. Editors:* — The kind reception which you have given to some of my observations in a previous number of your Journal, warrants me in offering you another, which, although imperfect, is nevertheless worthy of scientific attention.

About the 16th of January, 1855, I was called, professionally, to see Miss M. B., aged 18 years, newly arrived from France, who was affected with a well marked bilious fever, which subsided in about fifteen days under a saline purgative treatment; her functions soon resumed their natural condition, and the menses which had been suppressed during her sea voyage reappeared. Up to the end of April following, her health remained good, with the exception of slight colics which occurred daily without diarrhœa or constipation, and for the relief of which I was consulted.

I paid but little attention to these symptoms, which I attributed to nervous phenomena inherent to the age of the patient. On the 1st of May, the patient began to be affected at night with a dry cough, without expectoration or pain, with the exception of a moderate soreness in the epigastric region, depending upon the efforts of incessant coughing. I auscultated the patient, and discovered rude respiration without any *râle* whatever. A tickling sensation in the throat was the only symptoms which

gave origin to the violent paroxysms of coughing. I examined the throat which presented only slight redness, and prescribed light expectorants. The cough returning six days afterwards with considerable intensity, I had recourse to the preparations of opium, which produced immediate relief, but on the next day, she complained of a sense of heat about the eyes with considerable lachrymation, the conjunctiva was slightly injected, but the affection was not sufficient to prevent the girl from pursuing her ordinary avocations. This affection was attributed both by the patient and myself to the heat of the kitchen fire to which she had been exposed. Collyria of sulphate of zinc, and preparations of opium were first prescribed, afterwards emollients were ordered, and she was directed to avoid the fire. On the 7th of May, at my visit, there was handed to me a portion of calcareous matter, of the size of a grain of barley, which I was told had come from the patient's eyes the preceding night. I was at first inclined to disbelieve this statement, but the patient and her relations assured me that at the age of 15, a similar phenomenon had taken place, and that it had been also observed some ten months anterior to my visits.

This fact appeared so extraordinary, that I examined the conjunctiva through a good glass, when I found its folds filled with a secretion similar to that which had already been shown me, but the particles were so extremely minute, that they seemed inherent to the membrane, to which they gave a grayish tinge. The conjunctiva was dry and congested. The phenomena were extraordinary, I determined to suspend all local application, with the intention of observing closely the progress of the disease. On returning three or four hours afterwards to my patient, I was shown a half dozen concretions similar in nature to the first. The mucous membrane still presented the same appearance, with the exception that here and there scattered over its surface, were seen portions of calcareous matter of the size of a millet seed, perfectly detached from the membrane, and floating freely under the lids. With a moistened rag, I succeeded in obtaining several pieces which were similar in every respect to that shown me in the morning, and those handed me at the time of my visit. On seeing the patient again in the evening, I found the eyes so filled with the secretion, that it had the appearance of having been put there with the hand. The form presented by the secretion as seen in its position was crescentic, but on everting the lower lid, the mass broke into numerous fragments, and fell from the eye, leaving a red mucous membrane studded with a number of small white specks. The secretion seemed to be derived from the follicles

of the conjunctiva, and the tears appeared to dissolve it and form a milky layer covering the membrane. This phenomenon lasted three days, and I succeeded in collecting two or three ounces of calcareous fragments, some of which were nearly as large as a grain of black pepper, and composed of carbonate of lime.

The patient was kept in bed for three days, and as her sufferings were not sufficiently intense to require an active treatment, I merely applied lotions of cold water to the eyes. During this time, suspecting that the patient might be guilty of some deception, I visited her very frequently and at times when least expected, but I invariably found her eyes more or less filled with the secretion. On the 10th of May, as the inflammation seemed to be on the increase, I ordered two leeches to each side of the orbit, and on the next day, the secretion appeared to diminish in quantity. On this day, she was visited by a confrère with me, who agreed that it was a case presenting dacryolithes in a quantity and size hitherto unheard of. During the night of the 11th, the secretion ceased entirely, and the inflammation subsided to such a degree, that it was difficult to believe that the day before, the same membrane could have produced such vast quantities of the calcareous matter. During the time of the foregoing observations, I related the circumstances of the case to several of my colleagues, but unfortunately, but one (Dr. Turpin,) had the opportunity of verifying the fact. I determined, therefore, to wait for the manifestation of more phenomena, before communicating any further with other confrères.

I had not long to wait — on the 13th of May, I was called in haste, at 9 o'clock at night, to see the patient, who, according to the statement of her relatives, had, after passing a quantity of fragments of the substance in question, voided one of large size, the expulsion of which, was attended with so much pain, that they thought it best to inform me. Among the fragments discharged, was one the size of a filbert, that had probably given rise to the violent pain, and created the alarm. On examination of the fragments, it appeared to me impossible that they could have come from the bladder, and the idea that they were derived from the mucous membrane of the vagina and vulva, immediately suggested itself to me. As the patient seemed to be quiet, I deferred a rigid examination to the following day.

On Monday, the 14th, during my morning visit, the chamber pot was shown to me, containing about a dozen of the small fragments of the substance in question, besides a quantity of limpid yellowish straw colored urine ;



this condition of the urine had existed during the whole course of the disease, a fact of which, I had convinced myself by frequent previous examinations. I found the patient's eye lids not at all injected, and without the secretion, but the cough had returned during the preceding night with the same intensity as before, and without expectoration. I immediately prescribed an anodyne mixture, proposing to investigate what I supposed to be the vaginal secretion, as I had previously that of the eye. At 12 o'clock, on my way to see the patient, I meet Dr. S. Martin, to whom I communicated the girl's history, and requested him to see her with me. At our visit, I learned that the cough had diminished, and the secretion from the eyes recommenced, a fact, which we verified by everting the inferior lids, which we found to cover a quantity of chalky fragments of the size of a grain of barley.

From this time out, the vaginal secretion began to diminish, and that of the eye-lids continued to be produced in an intermittent manner, without any marked injection of the mucous membrane. At times, the secretion was produced every hour. Sometimes at intervals of scarcely ten minutes. The next day she was visited with me by Dr. Rancé, and we still found the eye-lids filled with the calcareous matter, in particles of different sizes, but in a less quantity than during the day preceding. The secretion evidently had a tendency to diminish, inasmuch as I had only been able since the preceding day to collect a half ounce of the fragments. The disease seeming to be on the decline, and wishing to avail myself of the time which it promised to last, I invited Drs. Brickell, Beard and Choppin to see the patient. These gentlemen complied with my invitation, and we went at ten o'clock. After having cleared the patient's eye completely of all particles, by the aid of a small ring, we seated the patient in the midst of us. We examined the eye every ten minutes, during two hours, but no secretion took place. At the end of this time, the patient, who had sat perfectly quiet upon her chair, suddenly got up, walked towards a cradle in the room which contained divers articles of clothing, stooped and seemed to search for something; at the termination of a minute or two she returned to her place and sat down. Five minutes had scarcely passed when turning towards us, she remarked that she felt the return of the secretion, and at the instant, we perceived in the internal angle of each eye two fragments, almost ready to drop from the lids. We opened the lids immediately, but could see no further secretion inside. I was at this moment so struck with the idea that the patient was deceiving us, that I

could not help accusing her of the fact. The gentlemen who were with me were also impressed in a like manner. We cleaned the eyes again perfectly, and after waiting for another half hour, we determined to adjourn to my office, telling the patient that if, in the course of a short time, she should have a further secretion, to come there immediately. As we were about to leave, I made use of a momentary absence of the patient to search the cradle already mentioned. I found, among other things, several handkerchiefs, which had been already used, and from which a few fragments of the chalky matter fell, on shaking them. Inasmuch as the secretion had been very abundant during the course of the disease, and it had not been uncommon to find the fragments scattered about the room, I did not attach great weight to the facts which I had lately observed, intending to wait, and by further observation, decide whether there was any deception in the case. We returned to my office, and about an hour afterwards the young patient came to us with her eyes filled with the secretion. We were so suspicious at this time, that the phenomenon did not create the same interest as before. Between this time and the next day, I visited the patient twice, finding each time the eyes more or less filled with the chalky matter.

On Wednesday, the 16th, I brought the patient to my office, and there, in the presence of Dr. Delente, and Messrs. Kohn, Landreaux and Ducatel, students of medicine, the eyes were carefully cleaned, and all particles removed. We then seated the patient in the midst of a circle formed by ourselves. The examination lasted two hours. The membrane secreted a small quantity of the chalk, but so little, that we with difficulty collected a dozen of the fragments about the size of a pin's head. The folds of the conjunctiva were covered with white specks, which seemed to be the extremities of small elongated bodies, the rest of which could be seen lying under the transparent membrane.

The balance of the day, the secretion continued in less quantities. Here my observations end. In spite of the ardent desire I had to push them further, I was forced to suspend them, owing to the conduct of a confrère, whose mistaken zeal frightened the parents, and led them to place the patient under his immediate direction.

The anticipation of the menstrual flow which would have taken place, led me to do little or nothing. From this flux I anticipated some positive modification of the disease, and I promised myself much interest in watching its course during this period, when, on Friday, a friend of the family

introduced the physician referred to, who asserted that there was cause for immediate action in the case, and at once prescribed. It was at this time that my observations ended.

I thought, gentlemen, that this affection was one of sufficient interest, considered either as trickery on the part of the patient, or as a true case of dacryolithes, (which is my opinion,) to be placed before your readers.

I remain, gentlemen, yours, respectfully,

E. MARTIN, D. M. P.,

*Visiting Surgeon, Charity Hospital.*

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## Excerpta.

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LECTURES ON CLINICAL SURGERY — DELIVERED DURING THE WINTER SESSION OF 1854-5 — BY JAMES SYME, ESQ., PROFESSOR OF CLINICAL SURGERY IN THE UNIVERSITY OF EDINBURGH. — *Lecture VII. — Fracture of the Thigh.* — The next case to which I shall call your attention is one of fracture of the thigh; and as we have not hitherto spoken of fractures, I will enter in a general way into the principles of their treatment, and without at present going into the subject of the mode in which the union of broken bones is accomplished, will consider at once the object and means of treatment. The object in treating fractures is to maintain the fragments in their proper place, and to keep them free from motion till reunited. The causes that induce displacement are, in the first place, the weight of the limb, which tends to cause motion of the broken surfaces, producing, for instance, in the inferior extremity a tendency to eversion of the foot, to obviate which the toes of a dead body are tied together. The great cause, however, is the action of the muscles, which constantly tend to draw their attachments nearer to each other, and are also liable to the additional contraction produced through volition and spasm.

The importance of considering muscular action in treating fractures was first particularly insisted upon by Percival Pott, whose writings on this and other subjects of practical surgery, I cannot too strongly recommend to your attention. Before his time, pieces of wood or iron, or some other unyielding material, were applied, to squeeze down the rising or riding end of the bone, as it was called, but which as Pott showed only appeared to project in consequence of the other end being drawn away from it. Pott himself happened to meet with fracture of the leg in his own person, and found great comfort in having the leg bent, on the journey from the scene of the accident to his own house. He saw that the relief which he experienced was due to the relaxation of the great muscles of the calf, which are not counterbalanced by any muscles of corresponding size on the front of the limb, so that their relaxation when the leg is bent, is not accompanied by any proportionate tension of the anterior muscles. Hence he was led to conclude that the great object in treatment should be to withdraw the obstacles produced by muscular contraction. Some who have imbibed his opinions at second hand, have supposed splints altogether unnecessary, but Pott was fully aware of the necessity of splints — not the short ones previously used to squeeze the riding end,

but long enough to extend beyond the ends of the broken bone. Keeping these principles in mind, let us proceed to the particular case of fracture of the thigh-bone.

Can anything be done by position of the limb, as in the case of the leg? Abduction of the limb would stretch the adductors and is therefore obviously improper, and the only position to be considered is that of the thigh and knee bent. On the front of the thigh are the extensors of the leg, which arise from the pelvis, and are inserted below the knee into the tibia; and on the posterior aspect are the flexors of the leg, which also arise from the pelvis, and are inserted into the bones of the leg below the knee. Now, if the thigh and knee be bent, the posterior muscles are stretched as regards their pelvic attachment, while they are relaxed with respect to their insertion into the leg, and the opposite holds good of the anterior muscles, which are relaxed towards the pelvis while they are stretched inferiorly over the bent knee; yet, from overlooking this most obvious fact, persons have fancied they have been doing their duty to their patients by bending the knee in case of fracture of the thigh, the result of which has been that the upper fragment has taken the direction of the trunk, while the toes have been everted from the weight of the limb, and the patient on his convalescence has discovered, to his great concern, that he has been left with a very unsightly and useless leg. All surgeons who have treated fracture of the thigh have experienced this difficulty, that there is no position that will relax the muscles, and accordingly various means have been resorted to for the purpose of counteracting them by extension. Hildanus employed what has been called his "jackstone," which was suspended by a cord that passed over a pully at the bottom of the bed; and others, again, have produced extension by means of the double inclined plane, of which you see an example, commonly known as M'Intyre's Splint, from having been invented by a surgeon of that name, in Newcastle; or sometimes Liston's splint, in consequence of a slight modification made by him. In using this apparatus, the leg is laid over it in the bent position: the foot is fixed to the foot-board, and the weight of the body keeps up extension behind, just as the jackstone of Hildanus made extension in front. The most effectual means of producing extension of the thigh is the long splint of Desault, which is a piece of wood, about as broad as the limb is thick, extending from the false ribs to below the foot, which is attached to its lower end, while a band passing round the perinæum has its ends carried through two holes in the upper extremity of the splint, and tied with any degree of tightness, so as to produce extension of the limb. But I have no hesitation in saying, that however heavy be the jackstone of Hildanus, or however firmly the patient be tied to the ends of the bed, however carefully the M'Intyre be applied, or however tightly the perinæal band be tied to the long splint, none of these means can prevent muscular contraction if the tendency to it continue, and if you obstinately persevere in combating muscular contraction with the long splint, you will incur the risk of ulceration of the skin of the perinæum, or mortification of the foot. Is the treatment of fracture of the thigh therefore hopeless? Is it true, as we have lately read in print, that a shortening of two or three inches is no discredit to the surgeon? I am happy to say that the case is very different, and that the result of our practice here shows thighs which have been broken, scarcely to be distinguished from the sound ones; only do not trust to overcoming muscular contraction by extension, but seek to prevent it by careful adjustment of the broken bone, and equable support of the limb in the proper position. If you succeed in these respects in the first instance, you will be surprised how easy the treatment will be afterwards; and instead of waging war against muscular contraction, you will have nothing to do but admire the tranquility of the patient, and the completeness of the cure.

The patient lately admitted is a boy, ten years old, and the fracture is in the upper third of the thigh. In children, fracture is usually in the upper third or the middle; in adults, commonly in the lower third; in old persons, generally in the upper part, through the trochanter or neck of the bone; but at all ages the treatment is extremely simple and uniform. A hair mattress is much preferable to a feather bed, which is so unequal and yielding, that I would rather treat a patient with a fractured thigh upon straw. Draw out the limb to its full length,



which you ascertain by comparing the distance between the knee and spine of the ilium with that on the sound side: feel that the fragments are, so far as you can judge, in proper position: then apply at each side of the thigh a splint of wood, pasteboard, or leather, extending on the outer side from the knee to the great trochanter, and on the inner side from the pubes to the inner condyle. It is of little consequence which materials be used; wooden splints such as these (Gooch's) are very convenient for private practice, as they can be easily cut down with a penknife, and at once adapted to a limb of any size, from a thigh to a finger; pasteboard and leather, however, are more accommodating, and can be fitted with greater accuracy to the limb. The splints are retained in position by bandages, which should not be rolled continuously round the limb, but applied at intervals, in the following way: a piece, rather longer than twice the circumference of the limb, is doubled, and passed round; one of the ends is passed through the loop, and tied in a bow to the other end. Three or four such "looped bandages" are applied along the thigh — three being sufficient, if the splints be of wood, while an additional one is desirable with leather or pasteboard, on account of their softness at the time of application. The advantage of this method of bandaging is, that the state of the limb can be observed for the most part without disturbing the bandage; and if it becomes necessary to make a fuller examination, all that is requisite is to untie the bows, when the splints can be separated from the thigh without its being raised from the bed. The spasmodic action of the muscles is continued, in consequence of their being pricked by the sharp ends of the broken bones. Now, when these lateral splints have been applied, although the muscles are compressed, and the risk of displacement diminished, yet, whenever the patient moves a joint of the affected limb he moves the broken bone, and it is therefore necessary to restrain the motions of the whole limb; hence the great advantage of Desault's splint, which, though powerless as a means of overcoming muscular contraction, is of inestimable value in maintaining the quiet of the limb; the patient cannot move his toes without moving the ankle also, nor the ankle without the knee, nor the knee without the hip-joint, so that if he moves his great toe there is a tendency to displacement of the fragments of the femur. It is curious that Desault has given a wrong representation of his own splint, and while describing it correctly as extending from the false ribs to a little below the foot, has figured it as reaching up only to the great trochanter. Unless it extend to the false rib, it has no power in restraining the motion of the hip-joint. In ordinary practice, the long splint is generally not made sufficiently broad; it ought to be equal in breadth to the diameter of the limb, or about four inches and a half for an adult, otherwise there is great difficulty in preventing eversion of the foot. The best means of fixing the limb to the splint is a sheet wrapped round the splint till a part equal in breadth to the circumference of the thigh remains unrolled; this is passed under the leg, and wrapped round it; after which its free margin is secured by strong pins to the part rolled round the splint, which serves also as a pad to the limb: it is also well to use a perineal band not to produce extension so much as to act as an additional means of insuring the perfect rest of the limb; the foot being fixed to the lower end of the splint by a handkerchief passed round the ankle just above the heel, and crossed over the dorsum of the foot, the ends being passed through the holes or notches in the splint, and tied upon its outer surface; a broad bandage carried round the chest and upper part of the splint completes the apparatus. The sheet is far simpler and more effectual than rollers applied from the foot to the groin: two or three bandages, six yards in length, are required for that purpose, and though they look very neat when first applied, they soon require re-application; the state of the limb can never be ascertained without either raising the thigh to take off the bandage, or cutting it up, and in the latter case the thigh must be raised in order to re-apply the bandage; whereas, when the sheet is used, on taking out the pins, and throwing the sheet aside, the limb is exposed undisturbed for examination. This apparatus has also the great advantage that it can be obtained extemporaneously; there are few patients, rich or poor, whose houses do not contain a piece of wood that would answer for a long splint, and a sheet or tablecloth to wrap round it.

I consider that it is a great error to teach surgery only adapted to hospitals. Complicated apparatus is, in my opinion, not as good as the simple; but even if the complicated means were the more effectual, the other also ought to be taught. But I would trust to the simple apparatus before you, rather than to the most complicated ever devised. Some surgeons who would go along with me thus far, make an exception in the case of fracture of the thigh in children, saying, that the fracture being in the upper part of the thigh, the upper part must be tilted upwards by the action of the psoas and iliacus, and that therefore the double inclined plane is preferable to the long splint for children. This, however, is a merely theoretical objection, and ample experience enables me to testify that the long splint is as applicable for children as for adults. I admit rare exceptional cases, however; for instance, I have found it necessary to use the double inclined plane when the knee has been ankylosed in the bent position, and again in one case where the leg had been amputated. Other cases also do occur, though very rarely. A man was once brought to this hospital with a simple comminuted fracture of the thigh, for which the long splint was applied; the patient, however, obtained no relief from it, but suffered such extreme agony, that it made him even get out of bed and drag the splint after it. I confess he met with but little sympathy, being supposed to be affected with delirium tremens. After a while swelling of the limb came on, followed by ulceration, and protrusion of the bone in the ham, and I was obliged to amputate the thigh. On examination of the parts we found that the fragments, instead of forming as usual a convexity forwards, projected backwards, and one fragment as sharp as a needle had penetrated the popliteal nerve, and obliterated the vein, so that the extreme pain, and the swelling of the limb were both accounted for. It happened that a patient came in soon after with fracture of the thigh, with fragments in the same position. My late house-surgeon, Mr. Bickersteth, told me that he could not make the bones lie straight with the long splint. We therefore used the double inclined plane with a perfectly successful result.—*London Lancet*.

**BATHS AND WASH-HOUSES.**—Cold is less fatal than dirt in the air and water of London, through which the cholera, dysentery, and other matters that induce zymotic diseases, are diffused. Thus, while intense cold killed 1,604 persons in the five weeks preceding the last, cholera destroyed 7,222 in five weeks of last year; and while cold killed 159 men and women of the ages of 20—40, cholera killed 1,909 at the corresponding ages. In all the religions of antiquity, before science had demonstrated the truth, that dirt is the cause of innumerable diseases, men had been taught by their great law-givers to regard personal uncleanness as something defiling and unholy; yet it is only *recently* that the artisans, and even the middle class of London, have in parts been supplied with baths, and with the means of washing linen easily, and at little cost. The last return of “the Committee for Promoting the Establishment of Baths and Wash-houses for the Laboring Classes,” shows that there are thirteen baths and wash-houses in London, at which 1,220,739 baths were administered for £17,062, and 421,101 washings of the linen of about 1,684,404 persons were performed for £6,096.—*Registrar-General's Report*, February 24th, 1855.

**ANÆMIA.**—The following elegant pharmaceutical preparation, which would so frequently be useful in anæmia, phthisis and diseases of like character, is the method of Mr. Bastick of London, reported to the London Medical Society. We recommend it to our pharmacutists as a valuable combination. Mr. Bastick proposes to unite quinine with cod liver oil in the proportion of two grains to the ounce. He styles the preparation “*Oleum Morrhuæ c. Quinine*,” and it is best made in the following manner. The bisulphate of quinine is dissolved in distilled water and a little sulphuric acid. The quinine is then precipitated with an alkaline carbonate. The precipitate is then treated with boiling alcohol, and the alcoholic solution is filtered and evaporated to dryness, when the pure quinine is procured and added to the oil. The mixture is heated in a sand bath until complete admixture takes place; the oil becoming perfectly transparent.

**FRACTURE OF THE ASTRAGALUS** — *By M. Vollmar.* — A strongly made country-man fell from eight to nine feet upon a threshing-floor. After the fall he was unable to stand any longer upon the left foot. Upon the removal of the boot, there was noticed a bony prominence in the arch of the foot; and the patient, imagining there must be a fracture, applied cold lotions to the part to reduce the swelling. The foot was found in its natural position with moderate flexion.

In front of the articular extremities of the tibia and fibula there lay, under the raised integument, a bony swelling, separated by deep depression from the outer malleolus. No hollow, from which the bony mass had escaped, could be detected; the tarsal bones seemed in their normal relations. Extension effected the reposition of the displaced parts, and in four weeks the patient was able to go about, proper bandages having been maintained to keep the separated portions of the astragalus together.

Fractures of the tarsal bones are very rare. In 1,024 cases of general fractures, Malgaigne noticed but 9. The os calcis is the most commonly injured. — *Ztsch f. Chir. u. Geburtsh.*

**CONGENITAL ABSENCE OF THE IRIS.** — *By Prof. Boeck.* — Professor W. Boeck, of Christiania, publishes in *Hebra's Zeitschrift* an account of a family, four members of which labored under this defect.

In the father, aged 52, there was no iris in either eye; the right lens was cataractous, the left cornea opaque.

In the aunt, aged 62, the right cornea was partly opaque, and there was incipient cataract: the left cornea was clear, but the lens of that eye was partially opaque and somewhat dislocated.

In the son, aged 17, both corneæ and lenses were healthy, and a reddish reflection was visible from the bottom of the eye. The patient was myopic, but his sight good in other respects; and he suffered no inconvenience from light. Both eyeballs were in constant oscillation.

The niece, aged 37, had the same deficiency of the iris; but her corneæ were nebulous and vascular. She could of course see but indistinctly, but had not lost the power of adapting the eye to different distances. This faculty, as our readers will remember, is understood to depend on the motions of the lens, effected by means of the ciliary body; and the latter may of course be present, and act well in the absence of the iris. — *Virginia Medical and Surgical Journal.*

**BILIARY CALCULI** — Dr. Bowditch, of Boston, believes that he has very much abridged the duration of the pain in such cases by lemon juice. The action is supposed to be similar to that of the nitro-muriatic acid bath. He remarks, also, upon the similarity of that disease with duodenitis, and the relief which lemon juice causes in the latter. — *Boston Medical and Surgical Journal.*

**PROPHYLACTIC INFLUENCE OF QUININE.** — Dr. Bryson, of the Royal Navy, publishes in the *London Medical Gazette*, many interesting facts in relation to the prophylactic influence of quinine. His observations were made in a report on the African Station, in 1847. The plan adopted was to add a strong spirituous solution of *amorphous quinine* to wine, in the proportion of four grains of the last to the ounce. Of this quinia-wine one ounce was given daily. The result was, according to Dr. Bryson's observations, that men who were exposed on land, or near the shore, where the miasmatic influence was highly concentrated, entirely escaped the disease; whereas, without such protection, the most fatal miasmatic affections would doubtless have occurred. In some of the reports failures of the medicated wine are mentioned, but in such instances there appears to have been either irregularities in giving it, or excesses on the part of the sailors, which would readily account for the failure.

These facts, although suggestive of nothing absolutely new, nevertheless, serve to remind the practitioner that quinine possesses prophylactic as well as curative properties; and that the medical profession, as well as others who are necessarily exposed in miasmatic regions, may avail themselves of the protective influence of the agent, and thus often escape severe attacks. — *Western Lancet.*



**INFLUENCE OF THE SULPHATE OF QUININE IN PULMONARY PHTHISIS.**—*By Dr. Muntendam.*—Dr. Muntendam publishes in a Dutch journal the results of his clinical experiments as regards the action of the sulphate of quinine in pulmonary phthisis. The observations, reported briefly by him, amount to twenty-two. Under the head phthisical, he has included patients exhibiting pulmonary tubercles, accompanied by febrile symptoms, and in whom the fever was found to depend upon the morbid processes existing in the lungs.

Dr. Muntendam states his deductions to be: 1st. That quinine, administered contemporaneously with the acetate of morphine—or in many cases alone—can in a great many instances prolong the patients' lives, and frequently produces a cure, unless another deposition of tubercles in the lungs re-awaken the morbid process.

2d. That it can prolong life, when the local process is not too extensive; that administered at an early period, especially to children, married and lying-in women, it may remove the disease, if all other circumstances are favorable.

3d. That the acceleration of the pulse does not diminish under the influence of quinine; at least that it will not be slower when assimilation is already restored, or certainly not when the patient is attacked with intermittent fever.

4th. That hyperemia, or congestions of the head, chest, or intestinal canal, do not contra-indicate its employment. In the majority of these cases, the fever disappears at the end of a certain time, returning, however, very readily: in other cases, slight but obstinate fevers require the use of quinine for several weeks. In acute cases, and in many chronic, the cough and expectoration increase at first, which is an indirect effect of the quinine, as is also the re-appearance of the menses: the exudation which takes place in the lungs, as œdema or fibrinous exudation—results of old congestion—is then expectorated, in a glairy mass, gelatinous in nature, more or less, frothy or serous, and in chronic cases the sputa are globular and nummulated.

5th. That Dietl affirmed with truth, that quinine exerted an insignificant influence on the organs of circulation.

6th. That sulphate of quinine administered continuously, and in small doses, does not occasion dyspnoea, diarrhoea, or any injurious effect.

7th. That sulphate of quinine deserves, in many cases of phthisis—not to say in all—to occupy the first rank as a therapeutic and curative agent.

8th. That there is no antagonism between pulmonary tuberculosis and intermittent fever.—*Revue Med. Chirg. frem Nederlandsch Lancet; et Journ. de Med. de Bruxelles.*

**CHLOROFORM IN THE INSERTION OF AN ISSUE.**—*By M. Danyan.* M. Richet said—I shall, finally, allude to the happy effects of the vapor of chloroform when thrown on ulcerated surfaces, which have been made known to us by Dr. Hardy, of Dublin, M. Moissenet and my colleague, M. Gosselin.

M. Danyan said—Having had to establish an issue on M. Roux, I made use of Dr. Hardy's apparatus. I employed the Vienna paste. I directed the vapor of ether for ten minutes on the nape of the neck, where the issue was to be formed; the pain was completely annihilated.—*Gazette des Hôpitaux.*

**A PAINFUL STUMP TREATED BY THE APPLICATION OF CHLOROFORM VAPOR.**—(Under the care of M. Larrey.) What appears certain is, that the vapor of chloroform freely directed to a painful point, immediately allays the sufferings of the patient. Thus at Val-de-Grace, after an amputation of the thigh, the stump becoming very painful, M. Larrey made use of Dr. Hardy's apparatus, and as soon as the vapor of chloroform came in contact with the wound, the pains were soothed. *Journal de Medecine et de Chirurgie Pratique.*

**CORYZA.**—Dr. Lombard states that the severe pains in the nose and frontal sinus, attended upon cold in the head, is easily relieved by the fumes of partially burned opium. One or two grains should be placed on a bit of sheet iron and held over a lamp, and the fumes inhaled through the nose.—*Brit. and For. Med. Rev. and Jour. Phar.*



ON THE TREATMENT OF GLEET.—By M. Ricord.—[M. Calvo, a private pupil of M. Ricord, has recently published in the *Moniteur des Hopitaux* reports of the lectures at the Hôpital du Midi during 1854. Here are some useful points in regard to the management of blennorrhœa, which we take from the number for September 12th, of the journal alluded to. On a future occasion we may make further extracts.]

Notwithstanding the most judicious treatment, blennorrhagia often passes into the chronic stage, and becomes the intractable disorder termed blennorrhœa, goutte militaire, or gleet.

When this occurs, it is proper in the first place to examine the meatus of the urethra; the lips of this orifice should be separated, and if any inflamed follicles are perceived, they should be incised with a cataract needle and cauterized with nitrate of silver.

If the discharge continues, it will be necessary to institute an internal treatment. The patient may take from three to six spoonsful of a solution of one drachm of citrate of iron in a pint of syrup of tolu, and drink freely of tar water, or of a decoction of uva ursi; I also frequently prescribe turpentine and Canada balsam in the dose of six-grain pills morning and evening.

The remarkable success obtained by means of the preparations of iodine, the iodide of iron particularly, in the treatment of chronic purulent discharges, induced me to employ these preparations in the treatment of gleet. The results have entirely satisfied me, and I regard the pills of the iodide of iron, preserved by the method of Gille, as the most efficacious medicament we can oppose to those chronic discharges, which are so rebellious as to cause the most experienced practitioners to despair of their cure. Four to eight pills may be taken daily; each pill contains one grain of the iodide.

Blisters to the perineum or the inner surface of the thighs sometimes have a good revulsive effect, and occasionally cuts short the disease; sea bathing has the same effect.

The following injections are suitable in this period of the disease: Rose water six ounces; sulphate of zinc and acetate of lead, each, half a drachm; or twenty or thirty grains of tannic acid may be substituted for the acetate of lead in the above formula. Or the patient may use an astringent injection composed of equal parts of water and Roussillon wine, with the addition of a little sugar. Here are two other prescriptions which are sometimes useful: vinous infusion of red roses, six ounces; tannic acid, one or two scruples—Mix. Distilled water, six ounces; iron filings, one scruple; protoiodide of iron, four to six grains. Mix, and take care not to shake the phial.

There are some obstinate gleet, uncomplicated with stricture, which can only be cured by the introduction of bougies. These instruments cause a mechanical irritation, and artificial gonorrhœa, which may be cured by cubebs and injections. The pewter bougies of Benique answer admirably for this purpose; they should be introduced gently, and left in place two or three minutes.

There is another plan which may be used as a last resource; it consists in cauterizing the whole canal of the urethra with Lallemand's caustic holder. The instrument is introduced until its extremity reaches the neck of the bladder; the canula is then drawn back and the cup of caustic is exposed and rapidly rotated as the instrument is withdrawn. There is considerable pain and scalding during micturition after the operation, but these symptoms rarely last long, and two or three cauterizations commonly put a stop to the gleet. But there are some cases which resist all the means I have described: in such, it is the surgeon's duty to recommend a tonic regimen, moderate sexual indulgence, residence in the country, mental diversion, and above all the patient should be exhorted not to think about his complaint, which is too often a cause of hypochondria and even of suicide in weak minded individuals.—*West. Jour. Med. and Sur.*

A BILL FOR THE ESTABLISHMENT OF A BOARD OF MEDICAL CENSORS has lately been introduced into the Legislature of Pennsylvania. It contains a number of objectionable features, with which we think the profession throughout the State should be made acquainted. The bill provides for the establishment of a Board

of Three Censors, to be appointed by the Governor, (and therefore, it is presumable, likely to be politicians,) who are to itinerate throughout the State, with authority to summon before them all practitioners of medicine, with a view to examination as to qualifications. Without a certificate from this Board no practitioner is hereafter to be allowed recourse at law to recover the amount of a bill for professional services, and a tax of *twenty-five dollars* is exacted for the certificate, which is to be available only for one year. An annual re-examination as to improvement in qualifications is also provided for by the bill, with an annual tax for certificate of *five dollars*.

It is scarcely possible, we trust, that so outrageous an interference with the rights of the profession in this Commonwealth can receive legislative sanction: but it will be well if those of our readers whose interests are affected by the scheme, will exert their influence to oppose it.—*Medical Examiner*.

**SUBSTITUTE FOR COD-LIVER-OIL.** — At the last meeting of the Liverpool Chemists' Association, Mr. Mercer produced a sample of oil imported into that town under the name of Shark-liver Oil. It possesses a peculiar interest in consequence of its low specific gravity. Until now, sperm oil, which has a specific gravity of .875, was the lightest oil known; but the specific gravity of shark oil was only .866. It came from Marseilles, and was stated to be procured from sharks caught on the coast of Africa.

**NOTICE OF A NEW TREATMENT FOR VENEREAL AND OTHER DISCHARGES IN BOTH SEXES, BY THE EMPLOYMENT OF LARGE DOSES OF THE SUB-NITRATE OF BISMUTH.** — *By M. G. Caby, interne of St. Lazare.* The numerous cures, M. Moneret has effected by large doses of the sub-nitrate of bismuth in case of diarrhoea, however obstinate, of dysentery and other maladies, induced me to apply the same treatment to venereal discharges, and the results, almost unhopd for, which I have obtained for some time, induced me to bring to the knowledge of my colleagues this method of cure.

1st. In Gonorrhœa, whether acute or chronic, in male subjects, I prescribed three times a day, an injection prepared with a certain quantity of water with as much sub-nitrate of bismuth as can be held in solution. The patient retains it about five minutes. In no case has it produced the slightest pain; a result of the insolubility of the sub-nitrate of bismuth. The continuance of the treatment has been from four to ten days. A number of these instances had resisted every other kind of treatment.

2d. The treatment is different, although not less efficacious in vaginal discharges, whether acute, or chronic, or simple, or even connected with ulcerations or chronic inflammation of the os uteri. It consist in the application by aid of the speculum and a simple pledget of lint, of the sub-nitrate of bismuth, dry and in powder. The only precaution is, to project a large quantity of the power upon the os uteri, and during the withdrawal of the speculum, upon the vagina and even on the labia. This application, producing not the slightest possible pain, should be made at least once a day, taking care to employ an injection to remove from the vagina the powder more or less moist, which should be replaced by a considerable quantity of dry powder.

This very simple treatment, which requires the concurrence of no other therapeutic agent, and which occasions not the slightest pain to the patient, operates, especially among females, with a promptitude so great, that the next day copious discharges have almost dried up. Another advantage possessed by the sub-nitrate consists in the modification, as rapid as inexplicable, of the redness, and even of the ulcerations on the neck of the uterus.

I hardly venture to say that all discharges, without exception, can be cured in this way: yet the results, already numerous which I have collected and purpose soon to publish in a memoir on this subject, leave no doubt of the efficacy of the treatment. I am, moreover, convinced that this has been already demonstrated by the studies of many physicians. — *Revue Med. Chirurg. from Monit. des Hopitaux*.

CASE OF HYDROCEPHALUS, IN WHICH THE HEAD WAS TAPPED EIGHT TIMES. Mr. Brown reports (*Assoc. Med. Jour.*) the case of a child, aged six months, suffering from chronic hydrocephalus, whose head was tapped eight times. At the first operation (August 16th,) six ounces of serum were removed, and the head tightly strapped; condition much improved; 18th, head was as large as ever, six ounces removed; September the 6th, head still larger, eleven ounces removed; 28th, twelve ounces; October 30th, abandoned tapping and inserted a seton of silk through the membranes of the brain, running it along the interior an inch before bringing it out; no immediate result followed, but large quantity of serum discharged. At the end of twenty-four hours there were vomiting, restless, and expression of pain; in thirty-six hours twitching of muscles were observed, with moaning and stupor. The seton was removed and the symptoms improved; the head was diminished in size, but soon began to enlarge; tapping was again performed, but the fluid quickly collected, and the case was abandoned to nature. The head continued to enlarge, and the child died. On examination, a large wash-hand basin of serum was removed; the containing sac was formed by the brain, completely unfolded, and no thicker than the dura mater. — *N. Y. Jour. of Medicine.*

APIOL, the immediate or active principle of parsley, has been suggested as a substitute for quinine. Its action is not unlike that of coffee (*Vivresse apiolique.*) Excitement from apiol is said to resemble, in every particular, an over-dose of quinine.

DYSENTERY. — Dr. Davidson, of Burmah, (*Lancet*, April 1854,) finds the free use of leeches to the anus the most reliable remedy he has ever tried in the dysenteries which are so common in that climate. The hemorrhoidal and mesenteric veins are thus emptied, and the disease cut short before running into the chronic stage. — *Virg. Med. and Surg. Jour.*

OVARIOTOMY. — Dr. Atlee, of Philadelphia, reports (*Am. Jour. Med. Sciences*) thirty cases of ovariectomy. This immense experience in the most desperate branch of surgery has been acquired in the short space of ten years. It is equally interesting to notice the large proportion of recoveries, — seventeen of the thirty cases recovered and thirteen died. In three instances conception occurred after the operation. — *Va. Med. and Surg. Journal.*

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## Editorial and Miscellaneous.

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### “OPERATION FOR RECTO-VAGINAL FISTULA.

“*To the Editor of the New Orleans Medical and Surgical Journal.*”

In the May, 1855, number of the New Orleans Medical and Surgical Journal, is the report of a successful operation for recto-vaginal fistula.

As it is not uncommon for other than generous motives to be imparted to those who may see proper to indulge in professional criticism, it may not be out of place to preface the following remarks with a total disavowal of anything savoring of an unkind feeling. We have not even the honor of a further acquaintance with the reporter than the perusal of his case has now established; consequently, there can be no concealed interests to subserve, no private pique to gratify. He who launches his craft upon the waters, should know that it is

seaworthy; the elements with which it must necessarily be brought in contact are not to be blamed should it spring a leak, or prove disobedient to the helm.

The reporter says that his case was one of recto-vaginal fistula; that "this aperture was at three inches from the lower end of the vagina, but opened about half an inch higher in the intestine;" that is, the fistula assumed an oblique direction upwards through the recto-vaginal septum, reckoning from its vaginal extremity. Again, "A crow quill was passed freely through the sinus," thus leading the reader to infer, as a matter of course, that the sinus was near about the size of a crow quill; though we will grant that it was nearly as large as *three* such quills. *Sinus*, in surgery, "means a long, narrow, hollow track, leading from some abscess," etc.—*Dunglison*. So that we may safely assert that the reporter had before him a recto-vaginal fistula, which, although situated pretty high up, was of very limited dimensions.

Again, he says, that he endeavored to effect a cure "by applying different caustics to the edges of the wound, and by introducing lint into the rectum," etc., "but these means failed." Again, "I thought of the suture; but the thickness of the parts, the difficulty of reaching them, the situation of the wound, and the great loss of substance by this time, caused me to abandon the idea. I resolved to operate in the following manner: I had a wood canula made with a deep groove on one side, which I introduced into the rectum. A grooved catheter was then passed into the sinus, until the end rested on the canula in the intestine. The bistoury was directed on the catheter, until the point came in contact with the conductor in the rectum. *The whole vagino-rectal septum and perineum were divided at one sweep*!!" (The italics and exclamation marks are ours.) Finally, after making our "blood run cold" with the above recital, the reporter administers a restorative cordial to our shocked nervous system, in the shape of the refreshing intelligence that the patient *absolutely recovered*!!

One of the most attractive subjects of the present day, is the treatment of the very affection above alluded to, together with its companion on the list of evils — vesico-vaginal fistula. Up to the present time, those affections have been considered so far beyond the skill of surgeons as to merit the term, "*opprobria medicorum*;" not that occasional cases of cure have not been sometimes reported, but for the reason that the large majority of cases have so far baffled all efforts of the surgeon, that no plan of treatment could be considered sufficiently successful to entitle it to the established confidence of the profession. Recently, however, a complete revolution in this state of affairs has been brought about by the brilliant success attending the operation of one of our American surgeons, (Dr. J. Marion Sims,) and there is the fairest promise that both vesico and recto-vaginal fistulae will soon be successfully and safely managed by intelligent surgeons throughout the land. So much, then, to remind the reader of the accepted or recognized principle of operating at the present day.

To the parturient female but one more serious accident can happen than that of complete rupture of the recto-vaginal septum and perineum, involving, of course, the sphincter ani muscles, and that is, rupture of the uterus itself. We believe



there is not a dissenting pen to this truism. The obstetrician has but to reflect for one moment on the general anatomy of the parts, then cast his mind's eye over the sickening results of this accident portrayed in the books, and there is no point in the long list of his duties to which he more assiduously directs his attention than the prevention of this dire accident. The not uncommon rupture of the frænulum perinei in primipara is in itself, by no means, a trivial accident, but when the *whole perineum* is ruptured, unbounded sympathy is to be accorded the miserable subject of such an accident. We repeat, that up to the present time, the large majority of such cases have proved incurable, and even now, we are not aware that Dr. Sims, or those who subscribe to his principles of treatment, offer certain relief to all who present themselves thus afflicted. Yet, for all this, shall we say that progressive surgery has, in order to effect the cure of a recto-vaginal fistula, of calibre sufficient to admit even *four crow quills, severed with the ruthless bistoury three-fourths of the entirety of the recto-vaginal septum, perineum, sphincter ani and all!!* Shall we say, that for the *chance* of relieving an affection, at the present day considered amenable to *safe* treatment, American surgery has indicted an injury next to the most calamitous that can befall woman!! Shade of the renowned Coan sage! hide forever thy timid hand, which was even afraid to "cut for stone;" yet cast an eye from thy peaceful retreat, and learn that scalpels oft are thrust ahead, "where even angels fear to tread."

But, in all candor, we should be delighted to acknowledge even the shadow of a good reason for the performance of such a terrific operation as this. Much more interest attaches to the report of cases, where the reporter assigns even a synopsis of the reasoning which induces the treatment adopted or operation performed. In the case before us, this feature is entirely wanting. True, the reporter does say, that he "attempted to produce adhesion, by applying different caustics to the edges of the wound, and by introducing lint into the rectum, so as to exercise compression, and at the same time keep the parts undisturbed; but these means failed"—a result which it would not have required a Solomon to foretell. What good was to be expected from cauterizing "the *edges* of the wound," a fistula, in such parts, we are at a loss to conceive; but more especially are we bewildered in the search for a shadow of hope to be derived from stuffing the rectum full of *lint!*

Moreover, the reporter says, "I thought of the suture, (we suppose he means the 'clamp suture'); but the thickness of the parts, the difficulty of reaching them, the situation of the wound," etc. It is evident, then, that he merely "*thought*" of the suture; he did not attempt to apply it; he did not even give another surgeon the opportunity of applying it; but, taking it for granted that the thing could not be done, dismissed the most approved theory and practice of the present day with a single "thought," and went to work with his bistoury, not only without authority, precedent or sound philosophy to back him, but with a dark future before him, through which he and his patient must go stumbling along, with hope alone on whom occasionally to rest their weary limbs. For the life of us, we cannot find in the history of this case a single feature which should for one moment have prompted the performance of such an operation. The case not only

presented merely the characteristics which *belong* to the affection, but the reporter's own history of it makes it fall far short of what would be considered an aggravated case of recto-vaginal fistula; and it is *only* in desperate cases that medical men are at all warranted in resorting to desperate measures of relief—even these measures themselves having a reasonable limit.

The reporter says: "This case has at least one interesting feature, if no other; it is the origin." To say that it possessed but this one feature of interest would be highly reprehensible; the case speaks volumes. 1st. "The hæmorrhage was slight;" a most remarkable interposition of the merciful hand of the Divine Providence! When we reflect on the extent to which the blood-vessels must necessarily have been severed by the bistoury, we cannot bring our mind to realize that "the hæmorrhage was slight." 2d. Being told that the woman was, previous to delivery, "in bad health, suffering from bronchitis, and was much emaciated," we are not only led to wonder that a healthy process of cicatrization was established, but we are staggered at the degree of boldness exhibited by the operator. The operation would have been sufficiently astonishing if performed on the healthy subject; but it is a very decidedly "interesting feature" in the case, that of the patient being "emaciated," and her "lungs being affected." 3d. It is a *very* "interesting feature" in the case, to know that no rational mode of treatment whatever is reported as having been adopted previous to the operation in question. 4th. A highly "interesting feature" in the case, is the fact, that the woman *actually recovered*, in spite of surgery. The tenacity of life evinced by the woman is truly extraordinary. And, finally, the *most* "interesting feature," (now that she really survives,) is the probable future of the woman. We sincerely trust that the reporter will keep an eye on her, and report not only her future state of general health, but the results of her next labor, should she be so unfortunate as again to become impregnated. With a cicatrix three inches long in the situation heretofore designated, and the amount of constriction *which must have taken place*, she certainly has her chances for ruptured perineum greatly enhanced.

No one can read the short history of this case, without being thoroughly imbued with the highest sense of admiration for that great gift to suffering humanity, the *Vis Medicatrix Naturæ*. What this subtile something is we know not; we cannot define it. Hippocrates pointed it out to us, however, and it has continued with us: always present to assist the well directed efforts of the skillful physician or surgeon, as well as to stand between malpractice and the patient. It is not infallible, however, and although it often comes off winner in the "high die" game of life and death, still, we too often find it impotent to stay the hand of destruction. *Post hoc ergo propter hoc*, is the rule of short-sighted philosophers, and we cannot for a moment imagine that the reporter would regard his case as furnishing a tittle of evidence towards the establishment of a new principle of treatment of the disease under consideration; on the contrary, the mind is forcibly led on to the conviction that the happy result in this case is only one more of those truly wonderful and inexplicable triumphs which we are but too often called on to witness on the part of Dame Nature, when, arising in all her injured majesty, she defies the best directed efforts to overthrow her supremacy.

A certain doctor once visited and prescribed for a patient. The disease was yellow fever; the prescription calomel. On returning to his patient next day, he found him dead. He made an entry on his book—“Calomel kills yellow fever.” A few days after, he paid a second visit to another patient, and found, that instead of taking his prescription, the patient had eaten heartily of bacon and cabbage, and was very much better. He forthwith made the entry—“Bacon and cabbage cures yellow fever.”

A case of recto-vaginal fistula is reported as having recovered entirely after undergoing an operation which *severed the entire recto-vaginal septum, perineum and all*. Is this, then, the most approved operation for recto-vaginal fistula?

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*Messrs. Editors.*—Notwithstanding the fact that some of our daily newspapers are constantly proclaiming our city in the enjoyment of almost unprecedented health, all who do not willfully close their eyes and ears to existing facts, *must* be aware that the cholera is in our midst, and is doing its sad work from one end of the city to the other; it is not true that “the cases are principally confined to the upper and lower portions of the city, and are mostly attributed to the drought, which forces a change from rain water to the meagre supply by the hydrants;” the disease is to be found every where throughout the city, and although it seems to attack more children and negroes, still it is to be found amongst high and low. Editors of newspapers may attempt to deceive the people in the country, and, by their strangely mistaken policy, may succeed in throwing the unwary of our population off their guard, but the sad experience of every hour in the day teaches the mass of our citizens but too truly that cholera is in our midst, and in all its strength.

Is it possible that an erroneous principle of a few irresponsible newspaper editors, founded and inculcated solely on the low basis of pecuniary profit, either to themselves or others, can carry with it such weight as to drive our city authorities from their propriety, and make them fear to acknowledge the truth, or to act in conformity to the exigencies of that truth? Is it possible that the great and ultimately powerful spirit of honest independence, which urges the tongue to speak the truth and advocate its claims, even at the risk of having that tongue dragged from its seat, is the price paid for the pitiful title of Alderman or Assistant Alderman? Does the assumption of such a title imply a total change in the moral man, and make him henceforth but the machine for the exclusive use of the low and sordid money hoarder? Is it true that our city council has no higher duty to perform than that of catering to the insatiable appetites of a parcel of cormorant office holders? Is it possible that the lives of the valuable population of a great city like this are to be weighed in the same scale with this most worthless class of society? Has health, the greatest boon which can be bestowed on man, no claims on the consideration of our Council? If there be one spark of that ennobling trait in the human character called philanthropy—if there be but one spark of it left amongst the combined authorities of our city, it should at once be fanned into an undying flame by the knowledge that the poor (and not only the poor) of our city

are absolutely suffering for that most indispensable and cheapest of all articles—water. We do not enter into details, but merely remind our city fathers, that, not only is the thirst of their children unquenched, but, for the want of a few gallons of water per diem, dirt and filth so disfigure their persons, that soon they will fail to recognise even their most natural offspring.

But are unquenched thirst and filthy persons the only evils to which attention should be directed? We have said that cholera is amongst us: we not only say that it is here, but we say that it is malignant in the extreme and prevalent. For the truth of this assertion, do not ask newspaper editors: but ask the physician, the apothecary, the undertaker, the sexton: ask the mourning friends in every street and alley in our city. Turning from this sadly satisfactory duty, let the Councilman cast an eye on the reeking gutters, the piles of filth in our streets; let him look into the yards of hundreds of our people, and see not only extraordinary accumulations of ordinary filth, but cesspools running over, and streaming into the little yards—the lungs of every habitation. Do not stop at Canal, or St. Charles, or C. rendel streets, where greedy contractors do make some *little* show of performance of duty; but leave the atmosphere of newspaper politicians and truckling office seekers, and visit the “back streets,” the abodes of the “bone and sinew” of our city, on whom in reality depends its prosperity, and, our life for it, he will return to his seat fully convinced that much may be done, and at a trifling expense, to mitigate the sufferings of our community.

But to come more closely to the point, may we be allowed to ask for what purpose, other than to carry out the system of quarantine adopted by our Legislature, was the present “Board of Health” constituted? In the officially published act of the Legislature we find the following clause: “The Board of Health shall have power to remove or cause to be removed any substance which they may deem detrimental to the health of the city of New Orleans, and the Commissioner of Streets shall execute their orders, whenever not in conflict with the ordinances of the city, or laws of the State: to pass and enforce sanitary ordinances for the city, provided the same are approved by the Council and published as city ordinances.”

Now, notwithstanding the direct inference to be drawn from this clause, that the present Board of Health was intended to act for this city, as well as the State at large, we find the strange proposition before our Council to organize *another* Board of Health, *another soup house* for the needy laggards of our very charitable community. And instead of such an iniquitous proposition being kicked out of the Council chamber, instead of its exciting the indignation of every member of the Council, it actually meets with that degree of consideration which promptly calls forth an able editorial in one of our daily papers in opposition to it. The individual who says he can see the remotest shadow of a reason for the organization of this proposed Board, sees it only through the medium of a preconceived necessity for the establishment of two more two thousand dollar offices to reward the zeal and untiring energy of self-sacrificing and disinterested citizens. As kind and benevolent as we believe our Council to be, we do not for one moment believe that they can be humbugged to this extent.



But to return to the subject of the real *bona fide* Board of Health. We frequently hear the question asked, "why don't the Board do something to improve the condition of the city?" A very important question, but readily answered. *They have no power to act.* The wording of the clause before quoted from the State act, is as well calculated to defeat itself as could have been desired by the most astute political trickster, and the individual who composed it deserves the everlasting gratitude of his order. Were we a member of the Board of Health we should consider ourselves about as fully authorized to declare war against a foreign power as to issue an order for the removal of any nuisance calculated to affect the health of the city. Let the "Board of Health" issue an order to the "Commissioner of Streets" to-morrow morning, and see whether it will be obeyed. They would be laughed to scorn by the very scavengers. New Orleans Boards of Health have never been so other than in name. A low spirit of petty jealousy has ever existed on the part of our City Councils, which has effectually repressed every dictate of common sense, of duty, pride or philanthropy. Our Councils have had such unbounded confidence in their own capacity to regulate all the minutia of city government, that a proposition or recommendation from a Board of Health has been considered rather an insult to their dignity. What inducements have they offered for zealous action on the part of the Boards, either as individuals or as a body? None whatever.

We find, then, that our Board of Health is ready and willing to act, if their masters will but give them authority. They refer us to the City Council. Strange anomaly! A Board of Health, having their regular meetings for the purpose of transacting business relative to the health of our people, yet powerless to act when and where the exigency of the case requires it. Out upon such pitiful mockery! Is it come to this, that even the public health, the lives of our parents, wives, children and friends, are either held subservient to the disgusting jealousy of an over important Council, or are staked in the great game of political preferment! If this is not in the reality a Board of Health, what is it? Is it, too, another political machine, formed merely for the purpose of absorbing an appropriation of money granted by our Legislature? Or is it conceded to be a body of honorable citizens called together for the very highest of all purposes, the working out the great and intricate problem, the preservation of the public health? A personal acquaintance with some of its members at once answers the former question in the negative; we know they would not lend themselves to a purpose so iniquitous.

The fault is not, then, with the so-called Board of Health, but with those who are fully competent to make them powerfully efficient, and yet, through some mistaken policy, stubbornly keep their hands tied.

Is it beneath the dignity of our Council to step forward and empower the Board of Health to look after the health of the city? They cannot be expected, individually or collectively, to tramp the city and look for dirt and filth, but a very few thousand dollars would enable them to appoint and pay agents (intelligent and trustworthy citizens—not political hackneys) to inspect regularly, and at short intervals, the streets, alleys and yards throughout our city, and not only see that each citizen performs his individual duty as regards the cleanliness of his premises,

but that the greedy and faithless Street Contractor performs his to the very letter, and thus renders a *quid pro quo*. The heavily taxed citizen has a *right* to ask that at least a pittance of the enormous levy that is made on him should go for this purpose, and he knows that it can readily be saved from the unreasonable sums paid to contractors of every description.

In the present state of its finances, we know that our city cannot afford to carry out a complete system of sanitary reform, but it *can* have the ordinary offal of each day removed from our doors, at a very reasonable expense, and there is no reason for the palpable neglect of this precaution. This is the age of progress; we have elected a "reform ticket" to administer our affairs, and in no particular is entire reformation more requisite than in the conduct of sanitary measures. We must confess to an abiding faith in the good sense and public spirit of our new Council, and we feel sure that they will offer the hand of confidence to our Board of Health, and so far from throwing any obstacle in their way, will volunteer every facility in their power to the advancement of the great cause of hygiene.

A PHYSICIAN.

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BOARD OF HEALTH.—The Board of Health of the State of Louisiana, composed of Dr. E. H. Barton, Dr. N. B. Benedict, Dr. Thomas J. Wilkinson, Dr. Samuel Choppin, Glendy Burke, James Graham, A. L. Trudeau, Th. Thayer and Captain Thomas J. Ivy, after some delay, have organized and elected Dr. S. Choppin as their President. Capt. Charles A. Labuzan was elected Secretary of the Board, and Mr. William Gillmore as Treasurer, at the Quarantine Station.

Dr. H. D. Baldwin has been appointed Resident Physician at the Quarantine Station on the Mississippi, and Dr. H. J. Richards elected to the Station on the Atchafalaya river. Both of these gentlemen are at their respective ports, ready to enforce the Quarantine law, which goes into effect on the first of June.

The Quarantine station on the Mississippi River, is situated about seventy-three miles below the city on the right bank. The site selected is excellent and eligible. The station on the Atchafalaya river is two miles below Pilot's Station, at the mouth of Wax Bayou.

For the present, in order to be prepared to meet the possible contingency of the approach of sickness, the Board has purchased two steamboats, one for each station to serve as temporary hospitals and dwellings, until the erection of proper buildings which have already been contracted for.

The improvements at the Mississippi Station will consist of two hospitals, officers' residence, sheds, fences, etc.

The Board have advised the Governor of the State to issue his proclamation declaring as *infected* all ports within the torrid zone, thereby subjecting all vessels, steamers or other crafts sailing from such ports, to a quarantine of not less than *ten days*, but more if found necessary.

Thus far the Board of Health have proceeded with commendable rapidity in the discharge of the important trusts confided to their care. Considering the circumstances under which they have labored, they merit much praise for their promptitude and energy. The public should not expect too much from the Board of

Health this summer, for several reasons; first, that it will require the experience of at least one season to mature and perfect, in a proper manner, the requisite regulations; secondly, owing to unavoidable delays the quarantine act only goes into operation from the first of June, while it should have been in force from the first of April.

The utility of quarantine in preventing the importation of yellow fever in our midst, being questioned by some, we have assurances that the quarantine which is about to be established, will be based upon such a footing, that of this trial a scientific experiment will be made, which must forever establish the foreign or local origin of yellow fever. This will be more than a recompense for the costs and sacrifices of its maintenance. As to its influence in excluding other diseases, such as small pox, typhus or ship fever, and others admitted to be contagious or infectious there is no dispute.

An awful responsibility will rest upon the Board if they do not use the powers vested in them in good faith, and with such light as science and experience have furnished at least attempt to avert the annual recurrence of a plague which has rendered life so insecure within our borders.

#### CHARITY HOSPITAL.

There have been several cases of interest within the past month, we subjoin the following:

A patient entered one of the medical wards complaining of severe sore throat. He had some laryngitis—was treated anti-phlogistically, but œdema of the glottis supervened and an operation became imperative. Tracheotomy was accordingly performed, the patient's symptoms much relieved. Unfortunately, however, the nurse who had charge of the ward observing that a portion of the drinks taken were coughed through the laryngeal tube, thought it would be well to administer them through this opening; this he accordingly did, filling the patient's trachea completely, a violent fit of coughing succeeded and the patient died some few hours afterwards. Such a fact as this should certainly have its weight in producing a reform in the system of nursing which now obtains in the hospital. There is no doubt of the fact, the nurses are for the most part totally incompetent to discharge their duties, and it is, in a charitable institution, a mistaken economy which hesitates at the expense of a few thousand dollars for the procuring of capable and faithful attendants on the sick.

A woman entered one of the surgical wards stating she had been beaten by her husband; this the latter denied. She had evidently been drinking lately, and from her history was in the habit of indulging in liquor. The surgeon ordered her a purgative and quiet; on the evening of the day she entered she was seized with convulsions of an epileptic form, which recurred at very short intervals, and continued throughout the night and following day, on the morning of the third day the surgeon saw her again, and diagnosed effusion on the brain. During the visit of the morning a fit of convulsions occurred, during which the symptoms of suffocation were so urgent, that tracheotomy was performed—respiration became immediately more easy, and the convulsions did not recur again. She died twenty-four hours after the operation. On post mortem examination, a large quantity of

extravasated blood was found situated at a point near the middle of the course of the longitudinal sinus, and external to the dura mater; on introducing a fine probe into the sinus at its anterior portion, the end could be protruded through a small opening which was situated in the sinus where it passed through the centre of the clot, from this point the hæmorrhage had evidently originated. The coats of the sinus were infiltrated slightly with blood. A careful examination of the bones of the skull showed there was no fracture. It is quite probable that the convulsions in this case were dependent on habitual drunkenness, and that during one of the fits, a rupture of the sinus occurred giving rise to the extravasation of blood and consequent death.

One more case of tracheotomy—a man entered the hospital complaining of some difficulty of breathing, he had marks of tertiary syphilis on his person and was put upon an anti-syphilitic treatment. A short time after he came into the hospital dyspnoea increased to an intense degree, and it was decided to perform tracheotomy—relief ensued, but he died six or eight hours afterwards. On examination œdema and congestion of the lungs with œdema of the glottis was found, the latter dependent on extensive ulceration of the larynx. There is but little doubt that had tracheotomy been made in this case a day or two earlier when the symptoms of dyspnoea first set in, and before the patient's forces had been exhausted by impeded circulation, that life might have been saved. It is a mistaken policy, in such cases, to defer an operation, when there is every probability that it must be eventually performed, until lesions have taken place which must necessarily terminate fatally.

In the eye wards three or four cases of some interest may be mentioned: A German entered ward No. 10, some time in the middle of May, for an affection of the eye; he presented the following symptoms: Great œdema of the right eye lids with exophthalmos, the eye being protruded to a certain extent from its orbit, and forced outwards and downwards; the cornea could be seen partially overlapped by the bulbular conjuction which was much infiltrated and congested, constituting the appearance denominated serous chemosis. The patient complained of a moderate degree of pain. In the right nasal cavity there existed an elongated tumor attached to the inferior turbinated bone, this was hard in consistence and apparently covered with mucous membrane. The right nasal bone and nasal process of the superior maxillary were partially absorbed by the pressure of the tumor. The patient's breathing was noisy, as in those affected with nasal polypus. On the right side and immediately over and parallel with the clavicle, was situated an elongated hard tumor about the size of a hen egg, slightly movable and not adherent to the skin. In the cervical regions of either side, the ganglia were much enlarged and indurated—both the tumor and the glandular affection were of long standing—a close inspection and rigid verbal examinations showed that he had never had syphilis. Patient's general health tolerably good. He gives the following account of himself: Says he is a wood-cutter, and worked at his business until about three weeks prior to his entrance into the hospital; at this time the inflammation in his eye began and he applied to his employer who gave him some salve, but getting worse he entered the hospital. He never noticed any affection of his eye, anterior to the time mentioned, and believes the sight was good.



From the appearance presented by the patient and his own history of the origin of his disease, it was at first thought that the phenomena observed were dependent upon an abscess of the orbit resulting from some cause unknown; in fact, the patient's story being credited, this was the only way by which to account for the principal symptoms, viz: the oedema, the exophthalmos, and the blindness; the latter evidently caused by pressure on the globe, for no species of tumor would be likely to form and produce such results as were here observed in the short space of three weeks. Acting upon the supposed existence of an abscess, a deep puncture was made in the orbit with a narrow bistoury; no pus was obtained. It was then that a suspicion began to be entertained, that the patient's account of himself was not correct; that his disease was of longer standing than he supposed, and that he had lost the sight of the affected eye without being conscious of the fact, as we have observed in other cases. On a further examination of the polypus of the nose, it was decided to be of a fibrous character; this fact, together with the indurated condition of the tissues of the orbit, which was made evident by the resistance offered to the bistoury in the search for pus, led to the supposition that the orbit was the seat of a fibrous degeneration, similar to that of the nasal cavity, and it was determined to operate for its removal, with the hope that the eye might be saved.

An incision was made in the median line, extending from a point half an inch above the superciliary ridge down the centre of the nose, and terminating on a level with the inner canthus of the eye; from this point was made another incision, joining the first at a right angle. The tendo oculi was then divided, and the superior lid reflected back in such a manner as to expose the inner half of the orbit. Attempts were then made to remove the diseased mass without implicating the globe, but it was discovered, that the attachments of the tumor were such, that it would be impossible to avoid the extirpation of the whole organ. This was accordingly done, and the diseased contents of the orbit removed as cleanly as possible. The cavity was filled with lint, and the patient removed to his bed. Much blood was lost during the operation, in consequence of the necessary division of the angular and supra orbital arteries, together with many small branches, which had become enlarged in consequence of the disease. On microscopical examination of a portion of the tumor, it was found to consist of a fibrous base, extensively infiltrated with encephaloid matter. The malignant nature of the disease was not suspected anterior to the operation. It would be interesting to discover whether the induration and enlargement of the cervical glands and the tumor over the clavicle are dependent on cancerous infiltration or not. The patient is not doing well, and there is great doubt whether or not healthy cicatrization of the parts operated on will take place.

The other operations in the eye wards, were one for artificial pupil, one for cataract, and another for strabismus.

The patient upon whom the operation for artificial pupil was performed, had been blind for four or five years in consequence of a syphilitic iritis effecting both eyes, producing occlusion of the pupil with capsulo-lenticular cataract. Two preliminary operations had already been performed for the purpose of breaking up



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NO. V.

Speculations on the Cause of Yellow Fever.

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[Continued from last No.]

In a former publication I remarked that the special cause of yellow fever is utterly unknown. This is unquestionably true, but it does not follow, because such is the case, that we may not obtain correct notions of the *general* nature of this cause. Though we may not be able to present the matter creating this fever so as to subject it to chemical analysis and determine its exact constitution, still, it may be possible, from a wide range of observation and experiment, and from analogy, to determine the *class* of substances it belongs to in natural history. Nor would such a result, could it be obtained, be an unimportant one. The strong bearings that it would have upon our views of the pathology, and consequently, upon the treatment of the disease, as well as upon the subject of hygiene, cannot be a matter of dispute: they must strike every one. The inquiry is one indeed of great difficulty, and I approach it, I hope, with the diffidence and caution such investigations demand. It will be seen, in fact, that I pretend to offer nothing original—my object is to bring to bear upon this important subject, the facts and arguments upon which my own opinion is founded, and which I believe to present the true view of the case. They are scattered through various works and embrace several sciences, and I shall therefore be obliged to use quotations freely. Were the paper written solely for medical men, these might, in a great measure, be spared, or at least greatly condensed; but convinced, as I am, that

yellow fever may be banished from our cities, my whole object is to put the subject in detail before the minds of the citizens generally, and of those who have control over municipal affairs, and who only have the power of applying the proper correctives. To this class of readers, medical writings, are of course, unfamiliar.

In beginning this subject it may be worth our while to sum up, in a brief and concise manner, certain general facts connected with it; and in so doing, I shall limit myself to those which are, I believe, undisputed, and several of which are of general notoriety.

1. The yellow fever of Louisiana only makes its appearance where persons are collected in crowds, as in cities, watering places, etc. Persons who live in the country and confine themselves to it, though they may be as unacclimated as any others, and as liable to the disease, are nevertheless, perfectly safe from attack.

2. The production of yellow fever in New Orleans cannot be attributed to marsh malaria, or to any kind of agents generated by swamps, marshes, pools, or standing water of any kind; since we know that unacclimated persons may reside in the midst of swamps, and enjoy perfect health, whilst the city is being ravaged by the pestilence. This is a truth known to most of the inhabitants of this city; and a striking example occurred within my own experience, during the epidemic of 1837. The New Orleans and Nashville Railroad Company owned a number of unacclimated negroes who were at work on the line of road traversing the swamp from the city to the lake. On the breaking out of the epidemic, I directed the overseer to permit none of them to enter the city. In one instance, the order was neglected: the slave was kept at work in the city for two or three days—returned to the Metairie Ridge, where the negroes were quartered, and was seized the same day with the fever. This was the only case that occurred among them.

3. The opinion that the disease is owing to miasm, brought by the north wind which generally prevails during the epidemic season, is, therefore, erroneous; since persons living in those very swamps, which the north wind traverses, are exempt from attack, provided they keep away from the city. On the other hand, the south and southwest winds, which prevail at other seasons, traverse, to reach New Orleans, swamps even greater than those passed over by the north wind.

4. The disease has been attributed to miasm generated by a part of the bed of the Mississippi laid bare at low water; but it is well known



that a healthier region, than that called the *Coast*, is rarely to be met with in any country. This tract of land lies immediately on both banks of the river; and it is well known that unacclimated persons, who spend the summer there, enjoy perfect health. Moreover, most of the inhabitants are themselves unacclimated, and are as liable to yellow fever as any other people, when so imprudent as to visit the city during an epidemic.

5. Persons who arrive in the city during an epidemic from the healthiest regions—even by the ocean—are subject to attack on the sixth, fifth, fourth, and even as early as the third day after their arrival. Cases of attack on the third day of their arrival were not uncommon during the epidemic of 1837. It is plain that those persons were subjected to the influence of some powerful local agent, which existed previous to their arrival.

From these facts, it is obvious that the yellow fever of New Orleans arises from causes peculiar to the city, and which are confined to it, or to it and its immediate neighborhood.

We can only form three possible conjectures with regard to the causes of an epidemic local disease. It may arise from the influence of climate, from inorganic poisons, or from organic poisons.

Some epidemics, the influenza, for instance, are supposed to arise from certain meteorological changes in the atmosphere; and they probably do; for they affect whole districts—nay, even continents—the country, as well as the city. Their simultaneous appearance in far distant places; and in other instances, their rapid transit from one place to another, forbids the supposition that they are propagated by the agency of man. The peculiar changes which produce such diseases are unknown, but it is obvious from what has been already said, that yellow fever cannot depend upon any such causes. The only conjecture capable of commanding attention for a moment, is, that the disease is caused by the long and continued action of heat upon the system. But the difference between the range of the thermometer in the city and country must be exceedingly small, and the fact, already mentioned, of persons being attacked a few days after their arrival in the city, at once overthrows such a supposition.

We must, then, in the next step of our inquiry, turn to inorganic poisons. But as yellow fever only appears at certain seasons of the year, and also only in certain years, our inquiry must be limited to the production of such poisons at such seasons. We cannot imagine, therefore, that any such poison is endemic, or pertaining to the nature of the

country. We cannot attribute it to the nature of the water we drink, or the food we eat. The same water and the same kind of food is used by all who live along the Mississippi; who never see the disease, but who infallibly suffer if they approach the city when an epidemic prevails. We must, therefore, limit our inquiries to such poisons as may be generated from the soil at particular seasons, and under peculiar circumstances.

Such being the case, we are confined to one class of inorganic poisons—the gases, which might be evolved from the soil or from water containing organic matter. But assuredly the cause of yellow fever is no known gas. It cannot be carburetted, phosphoretted, arseniuretted hydrogen; neither can it be carbonic acid gas, etc. These and many others of the known gases produce death; but their effects on the system are known. Never has it been observed that any of them produce symptoms like those of yellow fever. When fatal, their effects are rapid—generally instantaneous; when not fatal, the immediate effects, in general, pass away, leaving the patient sometimes merely debilitated. In these cases they may produce peculiar organic lesions. Many of them, besides, are appreciable by the senses—by their odor, for instance. Again—the chemist, in his laboratory, is frequently exposed to them, but if not of sufficient concentration, he escapes uninjured. No such symptoms as are observed in yellow fever have ever been known to follow. Besides all this, any one of the known gases can be produced in all parts of the world, but yellow fever is met with only in certain places, and within certain latitudes.

Driven from all the known gases, we must then presume that the poison, or cause, (if it be a gas) is one as yet undetected. But can we believe this? Analysis of the gases has been brought to such perfection that chemists are able to detect 3.15 parts of carbonic acid gas in 10,000 parts of atmospheric air. Assuredly were the poison a gas, it could not fail to be detected during the presence of epidemics like those which occurred in New Orleans during the years 1833-'37-'39-'41. But though the best chemists have been employed upon this subject, and in various parts of the world, and in the most unhealthy localities, they have found the proportions of oxygen, nitrogen, and carbonic acid gas nearly the same, and have failed to detect any gaseous body that could possibly produce yellow fever.

We must admit, indeed, that various accidental substances are being continually added to the principal constituents of the atmosphere. Thus: "volatile exhalations from the organized matter of the soil—products of

the volcanoes on the earth's surface, and of the artificial burning of fuel—gaseous fluids escaping from mines—vapors of volatile solid substances—hundreds of volatile oils from odoriferous plants—putrid volatile products of animal and vegetable decomposition—the muriatic acid arising from salt water in shallow lakes and lagoons\*—exhalations of men and animals—an innumerable multitude of substances ascending in vapor from manufactures and chemical processes—and finally, the volatile products of animal excrements; all these, are sources of pollution to the atmosphere, which thereby undergoes innumerable alterations in its composition.” † But it is obvious, from what has gone before, that several of these causes of pollution must be excluded from a theory which attempts to account for the poison or cause of yellow fever. This disease cannot arise from the products of volcanoes, nor the combustion of fuel, nor gaseous bodies escaping from mines, or from vapors of volatile solid substances, nor from the oils of odoriferous plants, nor from muriatic acid, nor from substances escaping from manufactures and chemical processes; because all these conditions are absolutely wanting in the city of New Orleans. There remains, then, “the volatile exhalations from the organized matter of the soil”—the “putrid volatile products of animal and vegetable decomposition”—the “exhalations of men and animals;”—and, above all, “the volatile products of animal excrements.” Here, then, we come upon our third category—organic poisons.

It is plain, for obvious reasons, that in this inquiry we must limit ourselves to such poisons, as may be produced by what is termed the spontaneous decomposition of organic matter—in other words, decomposition by putrefaction or decay. The time has long since gone by, when, to account for the prevalence of an epidemic, it was sufficient to say, that the wells of the city had been poisoned.

But organic matter is of two kinds, vegetable and animal. Does this poison arise from the decomposition of the first, or second, or from both?

That it does not arise from anything like marsh malaria has already been affirmed, and the affirmation is unquestionably true; for unacclimated persons, residing out of the city, even in the midst of swamps, are perfectly safe so long as they avoid the city. The whole Island of New Orleans is surrounded by swamps, but yellow fever is confined to the cities

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\* Mulder found free muriatic acid in the rain water of Amsterdam, which he ascribes to the decomposition of the chloride of magnesium, contained in the waters of the Lake of Haarlem, by the action of the sun's rays.—*Johnstone*.

† “The Chemistry of Vegetable and Animal Physiology:” by Dr. G. J. Mulder. Am. Ed., p. 94.

and villages. It might be supposed, however, that as in New Orleans we have many warehouses filled with a peculiar vegetable substance—sugar, and from which there is a constant drainage giving rise to offensive effluvia, the cause has generated from it. But the reply to this is, that on all the sugar plantations, the sugar-houses produce the same effluvia, without ever giving rise to yellow fever—that the decomposition of vegetable matter, under certain circumstances, may produce epidemic diseases, I have no doubt; but I am now speaking of the yellow fever of New Orleans.

In what respect does the city of New Orleans differ from the surrounding country, which is generally perfectly healthy when the city is being ravaged by the pestilence? It is situated upon exactly the same sort of soil, of exactly the same elevation, with all the surrounding circumstances the same. But it differs, in the want of vegetation; in masses of mankind being crowded together; in the want of free ventilation; in the accumulation of vast quantities, from year to year, of human excrements; (both urine and night soil) of those of other animals, and of dead vegetable and animal matter. In these circumstances, therefore, we must seek the cause of yellow fever.

This brings us to consider organic poisons of animal origin.

It cannot be necessary for me to prove, that the constituents of animal matter may be so arranged as to produce the most deleterious effects when introduced into the system. The existence of venomous reptiles and insects—of such diseases as small-pox, hydrophobia, etc.—sufficiently settle such a question.

But we must here pause and reflect a little upon the differences between vegetable and animal matter. In certain respects they differ most widely; in other respects, they are nearly the same. In other words, certain constituents of plants, such as gum, pure sugar, starch, woody fibre, etc., possess no nitrogen in their composition; and we have no reason to believe that the changes which these substances undergo ever generate epidemic diseases. But there are other principles, found only in exceedingly small quantities in most vegetables, but in large quantities in a small class; and this class is precisely that which gives sustenance to man and animals. The plants in this class furnish in fact the proximate elements of the tissues, which make up the bodies of animals. The animal makes nothing; it consumes. In the laboratory of plants of the class last mentioned, are really formed the proximate principles of the animal tissues. These



proximate principles are albumen, fibrine and casein; and they contain nitrogen; and not only nitrogen—they contain likewise sulphur. In short, during the putrefactive process, vegetables which possess these nitrogenized principles must give rise to products totally different from those which do not possess them; and these products must be the same (or similar) with those derived from the putrefaction of animal substances; that is, supposing all circumstances to be the same. We cannot, therefore, make an absolute distinction between vegetable and animal poisons, derived from putrefaction. Hereafter, therefore, instead of contrasting vegetable and animal poisons, we shall speak of the poisons causing epidemic diseases as *nitrogenized organic poisons*. This phrase, if it be limited to the products of putrefaction, will comprise those of both kingdoms.

The point, then, is narrowed down to this: Have we any proof that poisons may be generated by the putrefaction of nitrogenized organic matter? and if so, do such poisons produce any effects resembling the symptoms and lesions of yellow fever?

The following extracts will show that the question has been answered in the affirmative.

The first is a translation of some experiments of Gaspard, to be found in the second volume of Magendie's *Journal of Physiology*:

“*Exp. 14.* On the 19th of June, 1809, I injected into the jugular vein of a little bitch, half an ounce of fetid liquid arising from the simultaneous putrefaction of beef-meat and dog's blood. On the instant, the animal made many movements of deglutition, and very soon afterwards experienced dyspœa, *malaise* and depression. She lay on her side, refusing all food, and soon voided first her excrements, then her urine. In an hour's time, prostration of strength, gelatinous and bloody alvine discharges often repeated, dysentery, redness of conjunctiva. Afterwards, chest painful; belly hard and painful when touched; gradual extinction of strength; bilious, gelatinous and bloody vomitings. Death, three hours after injection. On opening the body yet warm, lungs inflamed, or rather engorged in a singular manner; but little crepitant, of a violet or blackish hue; with many ecchymosed or petechial spots, which likewise existed in the left ventricle of the heart, in the spleen, mesenteric glands, gall-bladder, and even in the sub-cutaneous cellular tissue. The peritoneum contained some spoonfuls of a reddish serosity, but the mucous membrane of the alimentary canal was most affected. That of the stomach was slightly inflamed; that of the intestines, above all, of the duodenum and

rectum, was considerably so—the color livid, with black points, and covered with a gelatinous and bloody substance, resembling the lees of wine or the washings of flesh. In addition, this inflammation was accompanied with a slight thickening of the tissues, and possessed a hemorrhagic or scorbutic appearance.”

“*Exp. 16.* The 14th of July, 1821, I injected into the right jugular of a large dog, two ounces and a half of a fetid liquid, that had arisen from the fermentation, for two days, of cabbage leaves, at a temperature of 20 deg. R. It was thick, not at all acid, and was mixed with an equal quantity of water. While injecting, the animal often swallowed, and before long commenced vomiting, which was frequently repeated, and soon fell into a state of depression. Some hours afterwards, great general uneasiness; pain in chest, upon pressure; respiration embarrassed, difficult and plaintive; appearance of peripneumony; then vomiting and great depression all day; at the expiration of nine hours, he had in the night a very copious and very fetid liquid stool, black as soot, analagous to the evacuations in *melæna*,\* and formed of a little excrements and mucus, with a great deal of apparently putrified blood. Some time afterwards, the dog had another stool, but it was merely muco-sanguineous.

“July 15th, depression more considerable; *adynamia*; recumbent on the side or vacillating walk; pulse, small and febrile; ardent and seemingly inextinguishable thirst; same aversion; urine natural and abundant enough; respiration free and weak. But what, above all, struck me many times during the day, was, that the pulsations of the heart would return at intervals with an extraordinary strength and noise, resembling what occurs in the highest degree of aneurism, combined with hypertrophy of the organ.

“The 16th, a little better; less depression; cessation of the disordered pulsation of the heart, but still ardent thirst; refusal of food; fever, and sometimes vomiting of drink.

“The 17th, the same condition as the night before.

“The 18th, symptoms aggravated; extreme debility; walk altogether tottering; eyes red, inflamed and blear; nostrils swollen and filled with mucus, obstructing the passage of air; mucous membrane of the mouth, violet-red and phlogosed. At mid-day, liquid stool of a whitish-gray

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\* *Note by Magendie.*—This fact, which Mr. Gaspard was the first to observe, is one most remarkable. It evidently points to the cause of the black vomits, which take place in yellow fever, certain typhus fevers, etc.

color, mixed with grumous blood of a purulent character and odor; death, during the night, at the termination of the fifth day of the experiment.

“Upon opening the body, the skin, sub-cutaneous cellular tissue and muscles presented the same appearance as after death from asphyxia from want of air, and did not appear exempt from inflammation. Conjunctiva, pituitary and buccal membranes red or violet, and covered with a thick mucus, very abundant and unusual; lungs of a gluey (*poisseux*) feel, slightly phlogosed in some spots, but crepitant enough. The left ventricle of the heart presented many brown spots or sort of ecchymoses, penetrating even into its tissue; it was, besides, of the color of lees of wine, which contrasted singularly with the natural color which the right ventricle preserved. But, in return, this last was in part filled up with a hard albumino-fibrous concretion, with a yellowish-white hue like that of fat, very homogenous, undistinguishable from the molecules of the injected liquid, weighing  $2\frac{1}{2}$  grains, almost entirely free, and only adhering to the ventricle by a finger-nail's breadth, apparently inflamed and somewhat torn. This concretion, with ramifications of the same color and consistence, extended into the pulmonary artery and into the superior vena cava, and also into the azygos, axillary, and even to the right jugular. Probably, it had been the cause of the violent pulsation of the heart, of which I have already spoken. The œsophagus and stomach appeared healthy, but the mucous membranes of the intestines, and particularly of the duodenum, rectum, and a small portion of the small intestines, was of a violet-red—colored, chiefly in longitudinal wrinkles and irregular patches, which gave a party-color to the outside of the intestines before they were cut open. Otherwise, this inflammation was without thickening of the tissues—without ulcerations, and much resembled ecchymosis or hemorrhage. In the duodenum, I observed many kinds of open blisters, whence a large quantity of sanious blood was made to flow by pressure on the neighboring mesenteric vein. The internal membrane of the rectum was still more affected, and its mucous glands were very swollen and apparent. This intestine contained puriform matters resembling those of the last evacuation. The other intestines contained mucous matter of a whitish-gray color, and very thick. The mesenteric glands appeared as if penetrated by blood and altogether inflamed. The gall-bladder, stained exteriorly by brown and violet spots, was filled with a black, thick bile, as ropy as melted glue.”

Gaspard performed many other experiments by injecting putrid water, and the results were, on the whole, the same as the above, the differences

being merely those of a minor character. No one, I think, can fail to be struck with the extraordinary resemblance of these symptoms and the post-mortem lesions to those of yellow fever. The characteristics of the disease, its rapid course, its hemorrhagic tendency, its peculiar lesions, are all to be met with in these experiments. We have black vomit, bloody alvine discharges, redness of conjunctiva, extreme tenderness over the abdomen, great and rapid prostration of strength, burning thirst, anorexia, etc., all so characteristic of yellow fever. In his other experiments he speaks of other characteristic symptoms—suppression of urine—intersusceptions of the intestines—the existence of fetid fuliginous matter in the bowels—ecchymosis of the mucous membranes—congestion of the lungs, etc. In short, hardly any symptom mentioned by authors as occurring in yellow fever, may not be found in these experiments; and it is the same with regard to the post-mortem lesions.

Now, it may be asked, whether other substances, when thus injected into the veins, do not produce similar effects? This question, experimental physiology has answered in the negative. Gaspard, and after him Magendie, have injected a vast number of substances into the veins of animals. Many of them cause death, but none of them the symptoms or post-mortem lesions described above. Those produced by ammonia come nearest to them.

The next extract, to which I call attention, consists of some remarks by Magendie. He observes: "That since medicine has existed, the pernicious influence of ponds, marshes, neglected harbors, and, in general, all places in which animal or vegetable matters were undergoing putrefaction, have been the subject of observation. Men, and frequently animals, inhabiting the neighborhood of these infected *foci*, were subjected to serious maladies, which authors have designated under very different names, such as the plague, intermittent fever, malignant fever, dysentery, cholera-morbus, typhus, yellow fever, etc. The bad effects of these *foci* of putrefaction are, therefore, well known; but the manner in which these putrefied animal or vegetable matters act upon the healthy individual; the nature of the change produced, and consequently the means to remedy it, are so many questions for which we have hitherto had but conjectures and hypotheses offered. Suffice it to say, they are still problems to solve. And nevertheless, what physician does not recognize their importance! What a vivid and unreflected light would be thrown upon the science of life, if we could treat of them in a satisfactory manner! At the present



day, we catch a glimpse of the possibility of treading this new path; and the probability of success will be sufficiently strong, if we can persuade ourselves that complicated questions in natural science can only be answered by the experimental method, and that they cannot at all be so by that of simple observation—a method, the only one which has been, and is, pursued by physicians.

“Palpable proof of what I advance may be found in the interesting treatise of Dr. Gaspard on putrefied substances. (*Journal de Physiologie*, tom. 2.) Using the experimental method, he succeeded, in some hours, in producing at will many of the diseases caused in man by putrid exhalations. The black vomit and black dejections are evidently the effect of an alteration in the blood, caused by the introduction of putrid substances into the circulation.

“I have repeated with the greatest care the experiments of M. Gaspard, and have pursued them with an eye to their application to medicine. I affirm, and it will be believed without difficulty, that his results are perfectly exact; in addition, I have observed, that different kinds of flesh have not the same activity in their putrefaction. The muscles of herbivorous mammalia appear less active than those of the carnivora. Putrefied oyster-water did not cause very violent effects; but the deleterious matter, *par excellence*, is putrid fish-water; some drops of this water, injected into the veins, producing in less than an hour, symptoms which have the greatest analogy with those of typhus and yellow fever. Death usually ensued in twenty-four hours, and upon opening the body, all the traces of a chemical alteration of the blood are discovered. The blood remains for the most part fluid; it has transuded through the walls of the vessels into the different tissues; particularly is it found to have traversed the intestinal mucous membrane; it, as well as mucus, is accumulated in the stomach and intestines, where it prevents all the intermediate hues between bright red and deep black. This phenomenon merits all the attention of physiologists; it leads us to examine the influence which viscosity of the blood exercises over the capillary circulation and over the exhalations; it seems to announce that the healthy state of the blood, in which its tendency to coagulation is very strong, prevents the transudation of this liquid through the walls of the smaller vessels. Under this supposition, a serosity, the most tenuous, could alone find a passage through these vessels in a healthy state of the blood.

“In following up these experiments, another fact struck me. The same

putrid water, so deleterious when injected into the veins, has no bad effect when introduced, even in a strong dose, into the stomach or large intestines of animals; nevertheless, I assured myself that it is absorbed by the intestinal mucous membrane; but it is not impossible that the mucus which covers the membrane and the pores of the small vessels, performs the office of a filter, which arrests the putrefied animal particles; permitting only those to pass which the water holds in suspension or even in solution. I propose to myself to make some researches on this new and interesting question. I have tried as yet but one experiment—the following: I took, in separate portions, two ounces each of putrid fish-water, which was cloudy by reason of the animal matters held in suspension. I filtered one of these portions through paper; it became nearly limpid. I injected the two liquids into two dogs, nearly the same with regard to age, size, etc. The animal which received the filtered liquid, experienced symptoms much less intense and much more prolonged, than that which received the unfiltered liquid. This last dog died six hours after the injection—the other survived two days.

“Filtration through a simple paper, then, has some influence, and we can reasonably suppose that it might have been more perfect—the putrid water losing all its deleterious qualities, as takes place when we employ filters of charcoal.

“The pulmonary mucous membrane and the tissue of the lung presented an analogous phenomenon. An equal quantity of the same putrid water injected into the veins, or introduced with suitable precautions, into the divisions of the bronchia, do not produce the same effects. Injection into the lung produces less serious consequences than injection into the veins. Such is not the case, however, with liquids which cannot be decomposed; or, to be more exact, which cannot be modified by filtration; the effects are nearly the same, be they introduced into a vein, or injected into the trachea.

“There is a kind of research which might conduce to very important results: it is, to study upon animals the effects of the effluvia or miasms which spring from substances in putrefaction.

“During the course of the last summer I made some trials on this point. I placed a cask in such a way that its body might contain putrefied substances, and so, that over these an animal might abide, supported by a grating with a double bottom, and remain thus exposed to the miasms which were continually escaping.

“At first, I placed some pigeons, rabbits, and Guinea pigs on the grate. Their food was choice and abundant. None of these animals were affected, although they sojourned here nearly a month, and although, at the bottom of the cask I had kept up a very active *focus* of putrefaction. I then put upon the grating a very sound dog; he was there well fed—visited often—and caressed, in order that he might patiently support his captivity. The four first days he retained his position well enough, but afterwards began to grow thin; and although he preserved his gayety and his appetite, he died emaciated at the end of ten days. His case offers then no sign which recalls to mind the effects of putrid substances injected into the veins, and above all, there was no black vomit; but at last he died, evidently from the influence of the miasms which he had inhaled, and which he had swallowed with his food.

“Upon opening the body there was an almost total absence of fat—some food in the stomach—and chyle, contained in the lacteal vessels and thoracic duct. The mucous membrane of the intestines was inflamed, but much less so than in cases in which putrid substances had been injected into the veins.

“I have often performed this experiment, and the results have not sensibly differed, unless we except the period of death, which, in one case, did not not arrive until the twentieth day.

“I must not pass over a remarkable exception. A dog, three years old, (*griffon*) experienced no derangement of health during a sojourn of six weeks in the cask—he was, as we may say, acclimated. In order to assure myself if he were really beyond the usual conditions relative to the effects of putrefied substances, I injected into his veins a quantity of putrid liquid, more than sufficient to produce serious lesions, and I was not a little satisfied to see him experience almost no effect from it.

“From the preceding facts, it is seen, that putrefied liquids, when they are injected into the veins, cause death, or effects which have the greatest analogy with those of yellow fever and of typhus; that the prolonged respirations of putrid miasms produces death also, but in a period much longer and with symptoms which differ from the diseases I have just named. What can be the cause of such a difference of the mode of action in the same substances? Why this diversity in their deleterious properties?

“It would be so much the more important to be able to answer these questions, as therein lies the whole difficulty, relative to the epidemic diseases which have recently occupied the public mind.

"Among the conjectures which may be offered, there is one which merits particular attention. We may presume that different atmospheric conditions, and particularly temperature and moisture, ought to have a great influence upon the mode of action of putrid miasms. I have begun some observations on this point: I will give an account of them on another occasion." \*

Following this, in the same volume, appears an interesting paper from the pen of M. A. Demouslins, which is here translated:

OF THE ANATOMICAL CONDITION OF THE SKIN AND SUB-CUTANEOUS  
CELLULAR TISSUE IN YELLOW FEVER.

"On the 3d of December, 1821, I read to the French Institute some observations upon the anatomical state of the skin, and of the sub-cutaneous tissue in yellow fever, and from this note the following conclusions may be drawn:

"1st. That in yellow fever there is no increase in biliary secretion.

"2d. That the mucous surface of the intestinal tube exhales the matter of black vomit, both when thrown up by the stomach, or passed per anum.

"3d. That the yellow color of the skin is the effect of a peculiar elaboration from the blood contained in the capillary vessels of the dermoid tissue, in which is established a congestion or a fluxionary movement analogous to that which takes place at the same time in the hemorrhages of the intestinal membranes.

"4th. That when the texture of the skin is very dense, hemorrhages do not occur.

"5th. That the yellow tinge, almost always preceded by petechiæ, is in truth nothing more than a sort of general ecchymosis.

"6th. Lastly, that yellow fever is nothing but a congestion or a sanguineous fluxion taking place at the same time upon the skin and mucous membranes, chiefly upon that lining the digestive tube, with different degrees of intensity upon each of these membranes, which are, moreover, equally permeable by the blood."

"Relative to the origin of those matters rejected by the stomach, Dr. Ferrih has remarked that the black vomit did not come from the liver, as had been supposed; for after death, he had found these matters in the stomach of patients who had had black vomit, and where the pylorus

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\* "*Journal de Physiologie*," tom. 3.



was completely obstructed by schirrhus. As in the case in question, yellow fever did not deviate from its ordinary type, we cannot, from a single exception, argue against those facts which have been offered. The matter of black vomit, then, does not come from the liver; it is, then, the product of a vascular exhalation. This necessary conclusion becomes then a certainty, if, as has been affirmed, Dr. Ffirth found this matter perfectly formed in the arteries of the stomach.

“In relation to the cause of the yellow color of the skin, and the origin of the coloring matter which produces this tinge, I have already observed, that Autenrieth and others have seen the serum of the blood assume a yellow color in patients free from all bilious complications; that in old persons the skin sometimes assumes a yellow color in full health; that M. Breschet, in the icteric cadavers of the new-born, has witnessed no sign nor augmentation of hepatic secretion, nor any change in the bile. To this I will add, that the chemical analysis of the blood of the newly born, jaundiced for fifteen days, made by M. Lassaigne, he was unable to detect, either in the serosity, or in the fibrin, or in the cruor, a single atom of any of the elements of the bile, or any of that resin which imparts to the bile its peculiar color. I have already observed that the icterus of new-born children is sometimes partial, and that foundlings are much more exposed than others, because they are not so well preserved from the effects of cold, in passing from the uterus into the world; that M. Dalmas has often failed to find anything morbid in the liver of yellow fever subjects, and the yellowness first shows itself in striæ along the course of the great blood-vessels, and not upon the conjunctivæ as in hepatic icterus. Finally, I have observed that some men are universally icteric; that biliary diseases are not more common among this class than others; that this color depends either upon the chemical constitution of the blood, or upon the peculiar elaboration which this fluid undergoes in the capillary system of the dermoid structures—structures known by the name of the mucous bodies of Malpighi; that finally, certain conditions, either physiological, as pregnancy, or pathological, determine in the different species or races of men, anomalous coloration of the skin, both local and universal.

“From all these facts, it follows that the yellow color of the skin does not require the previous increase of the biliary secretion, since this color may coincide with the permanent physiological state of the individual, or even with entire species; nor does it require a deviation or the transportation of this humor out of its ordinary channels, nor its transfusion into

the blood, since in the case of icterus, chemical analysis cannot detect in this liquid a single atom of the materials of the bile. Since I read my note to the Institute, experiments on dogs, upon the production of diseases quite analogous to yellow fever, both in reference to the course of the phenomena and the pathological state of the organs and the fluids of the body, have been published in the physiological memoirs of M. Gaspard, upon purulent diseases, etc., inserted in the January number for 1822, of the Journal of Experimental Physiology. The conformity, in all respects, of his fourteenth, fifteenth, and particularly of his sixteenth, experiments, with the case which I have reported, is too striking to be passed by in silence.

"The following is my observation: In July, 1815, a man arrived at Havre, from Fort-Royal, Martinique, after a short passage of twenty or twenty-two days; on the day succeeding, he came to Rouen, fell sick, and was carried on the same day to the Hotel-Dieu, when he suffered successively with pain of the head, delirium, great agitation, brilliant redness of the eyes, with alternate loss and recovery of sight; hiccough; slight hemorrhages from nose, mouth, anus; a perspiration which tinged his linen yellow; ecchymoses; on the third day, general and sudden attack of jaundice; coma—partial convulsion of all the limbs and of the face; carphologia; on the fourth day the agitation increased, and on the fifth he died.

"In the sixteenth experiment, M. Gaspard injected into the right jugular vein of a dog, two ounces and a half of a fetid fluid, thick, free from acidity, obtained from the stems of fermented cabbage. During the injection movements of deglutition, then vomiting and prostration took place; some hours afterwards, great *malaise*, chest painful on pressure; symptoms of pneumonia, then renewed vomiting and great prostration at the end of nine hours; during the night, copious liquid stools and very fetid, as black as soot, resembling the dejections of *melæna*, and composed of a small portion of excrements and mucosity, with a great deal of decomposed blood.

"Some time afterwards, a second stool, of a muco-sanguinolent character. On the 15th July, (the second day) increased prostration, adynamia—burning thirst; disgust; urine normal, repeated returns of feeble action of the heart, with irregular action. On the 16th, prostration less; cessation of palpitations; but always intense thirst, loss of appetite, and sometimes vomiting of drinks.

"On the 17th, same state; 18th, extreme debility; excessive thirst; eyes red, inflamed and filled with tears; nose tumefied, obstructed with mucosities; mucous membrane of the mouth red, violet-colored and phlogosed. About the middle of the day, whitish dejections with some decomposed fetid blood; died during the night, at the close of the fifth day. We observe that all the external tissues were highly injected with blood; their surfaces exhaled a remarkable and very abundant fluid. If we observe no discoloration of the skin, it was because the hair prevented this examination, since the post-mortem convinced us, that it too participated in that excessive injection of the capillary vessels of all the membranes. It is not necessary to make any comments on the resemblance of the symptoms, their nature and duration during the career of the case.

"But let us compare the state of the organs. I opened the body five or six hours after death; it was yet warm, the skin yellow, particularly about the cheeks, axilla, groins, and generally wherever the sub-cutaneous cellular tissue was most abundant.

"The skin, when incised, poured out, as in the living, small drops of blood. We may observe something similar, but in a less degree, in cutting the skin when blisters have been applied or suppuration has commenced. Hardly had the last layer of the integuments been divided and the instrument reached the cellular tissue, when gases escaped through the cut; the cellular tissue then shrank; its layers were injected with a net-work of very fine vessels, resembling an inflamed conjunctiva. Its color was of a brownish red. It was in the region of the parotid, that I observed this color; it was the same thing with regard to the axilla and groins. The stomach, the heart and the colon contained the same black and putrid matters, which had been discharged by stool and vomiting.

"All the intestinal canal, the stomach, and especially the large intestines were of a brown red color, with here and there patches of a deeper color; the liver presented nothing peculiar, and the gall-bladder, without being distended, contained a brownish yellow bile.

"At this time we had in the hospital some cases of typhus, arising from the army after the battle of Waterloo. The bodies which I opened before being cold, likewise contained gas, distending the cellular tissue, and escaping with considerable noise through the small punctures made in the skin—and also a high degree of injection in the layers of cellular tissue and a discharge of blood from the divided surfaces of the true skin.



"On opening the *warm* cadavers in the 14th and 15th experiments of M. Gaspard, in which the morbid phenomena resembled those of the 16th experiment already cited, there were found many ecchymosed or petechial spots in the tissue of the heart, of the spleen, mesenteric glands, of the gall-bladder, and *even in the sub-cutaneous cellular tissue*. Inflammation of the digestive mucous membrane, lined with a gelatinous and sanguinolent layer, similar in appearance to the lees of wine, and the washings of flesh, was accompanied with moderate thickening of the tissues, and presented hemorrhagic or scorbutic appearance. In these two experiments the injection was composed of putrid animal matters.

"On inspection of the body, in the 16th experiment, the skin, the *sub-cutaneous cellular tissue*, and the muscles presented the same aspect as after death from asphyxia, and were not entirely exempt from inflammation.

"The conjunctivæ, pituitary, and buccal mucous membranes were red, or of a violet color, and lined with a *thick coat of mucus*, of an unusual appearance.

"A large fibrinous clot of yellowish white filled the right ventricle, extending into the pulmonary artery, the veins, superior cava, azygos, etc., and explains the powerful palpitations observed during life. The intestinal mucous membrane, particularly near the duodenum and rectum, and less so in the small intestines, was of a violet red, inflamed in longitudinal lines, and *irregular plaques* which even tinged the outside of the intestine. Again, this inflammation was accompanied with thickness of the tissues, free from ulceration, and bore a marked resemblance to ecchymosis and hemorrhage. In the duodenum there existed open blisters from which much dark fluid blood was made to escape in pressing the neighboring mesenteric vein. The last phenomenon is similar to that reported by Dr. Ffirth. To comment upon the striking similitude between these facts and those reported by M. Gaspard would be superfluous. The difference between them are not more striking than between observations taken by different physicians in the same epidemic, either of yellow fever or any other disease.

"In the cases reported, the cause of all the pathological phenomena, and the subsequent organic changes is evident. It is the introduction into the blood of putrid matters. In the case of yellow fever and of typhus, the same agents produced the same or similar effects. This fact has already been demonstrated. Only in epidemics it is under another form, by other vehicles, and upon other surfaces, that the agents act. But through these



surfaces they do not less promptly enter the circulation. It is, however, of little consequence what part of the circle is described — it suffices to know that the blood and the putrid matter are brought into contact.

“The chemical changes brought about in the condition of the blood, a species of hemorrhagic inflammation of all the mucous and cutaneous tissues, and the different exhalations of blood more or less elaborated, which takes place, constitute the characteristics of the cases in point. Nothing is seen which might lead to the supposition that there is either an increase of hepatic secretion, or that the biliary fluids escape from their accustomed channels and enter the circulation. I persist then, in the conclusions which I have already stated, and insist upon the importance in pathological physiology of the two anatomical facts, which I was not perhaps the first to describe, because I made the post-mortems too late.

“The following are the facts: 1st. The exhalation of gas in the subcutaneous tissue, and 2d, the injection of the layers of this tissue, and of the entire thickness of the true skin by blood, which does not usually penetrate these tissues; injection whence results, as well as from the chemical alteration of the blood, and a peculiar elaboration effected in the skin, the yellow color of the membranes.

“This fluxion is evidently the same in regard to the dermoid system as are the hemorrhagic congestions upon the neighboring mucous surfaces. If there is no hemorrhage from the skin, it is because its texture is too close to allow this fluid to escape. I insist upon this injection of the vascular net-work, pervading the cellular and cutaneous tissues — a system of vessels through which the blood cannot penetrate in a state of health; for it is important, in a therapeutic and physiological point of view, not to confound a mechanical phenomenon, such as the effusion of blood by contusion with chemical phenomenon, such as the change in the condition of an altered fluid, either from the effects of a particular elaboration which is impressed upon it by the system of vessels through which it circulates, or by some reagent which is introduced into it.

“We may then, from the facts which I have stated, deduce this singular consequence; (viz. :) an explanation of the frequent alternations of epidemic yellow fever, with yellowness and vomiting, with epidemics characterized by vomiting without jaundice. It is clear, that, when the fluxion towards the mucous membrane is very great, and the predominant symptom, then the skin is less injected; and *vice versa*, when this fluxion predominates in the skin, the mucous membranes should be less likely to be the seats of an active hyperæmia.”

[To be continued.]

## Observations upon Vaccine and Variola:

*By WARREN STONE, M. D., Prof. of Surgery, University of La.*

Notwithstanding the general immunity from small pox, given by vaccination, there is much distrust in its efficacy, and the public mind is greatly disturbed whenever small pox makes its appearance. I feel confident that vaccination, properly conducted, affords complete security against small pox, and even varioloid or the modified form of it. In order to make use of a remedy or agent, to the utmost advantage, it is necessary to understand its mode of operation and the laws that govern it. It is a fact that I believe is not disputed, that the vaccine matter, or poison, is the same as the small pox poison, only modified by the animal. Wilson, in his late work on "*Diseases of the Skin*" states that he considers it established, that small pox may be transmitted to cattle, both by contagion and inoculation, and that the vaccine matter is the result. He refers to an essay read before the College of Physicians, in London, in 1828, in which it is stated that, vaccine matter having failed in Egypt, medical men were led to make experiments by which it was discovered, that by inoculating the cow with small pox matter from the human subject, fine active vaccine virus was produced. This fact was discovered by an American, I think, before the above date.

The vaccine virus, then, is simply small pox matter, modified by passing through the animal, and as the disease is rendered so much milder by this modification, it is reasonable that the effect in destroying the susceptibility to the disease in the system should be less than the disease in its original purity and severity. Cases are recorded of small pox being taken the second time the natural way. Inoculated small pox, being much milder than the natural, gives less immunity from second attacks, and the vaccine disease gives still less, it being still milder; but all the second attacks are modified in various degrees—some so mild as scarcely to be recognized, while others amount almost to distinct small pox. Experience shows that a single vaccination gives entire immunity in a majority of cases, and makes the disease extremely mild in a large majority of the remainder; and my experience goes to show that repeated vaccinations give entire immunity in all cases, if properly made, and at the proper time. In addition to the contagious character of small pox there is, occasionally, an epidemic

character in the atmosphere, which not only gives virulence to the disease, but favors its spread, and it is at these times that varioloid or modified small pox prevails, and patients will take it by a distant exposure, and even without being aware of any exposure, and the vaccine virus takes more promptly and with greater severity. There are occasions, also, when small pox scarcely seems contagious. I have had fair opportunities to observe this in the Charity Hospital. When I was House Surgeon there was no special place for small pox in the city; consequently, when a case occurred in the house it was necessarily allowed to remain. I used to place them in what was termed the cells, which were simply divisions of one of the wards, on the lower floor, into single apartments, made so as to secure unruly patients in; and all the ventilation was from within, or from the common hall. Isolated cases were frequently treated in these cells when there was no epidemic influence, and no case of either small pox or varioloid would occur among the other patients. But, again, when small pox was prevailing, cases would occur in remote parts of the house, and in cases, too, that could not have left their beds and approached nearer to the cells. There is, evidently, a great difference in the susceptibility to the action of the virus in different individuals, and it is much more obnoxious to some than to others—a difference that does not depend upon the difference in natural vigor of the system. The same person is more susceptible at one time than another to the influence of the virus.

A nurse in the hospital who had been attending small pox patients at a time when the disease was epidemic, was taken with high fever and strong symptoms of small pox, but he declared that he had been several times exposed to it, and at one time while in the service of the Colombian revolutionists he was detailed to the hospital service, in the city of Caraccas, where the small pox was raging, and the hospital filled with it. He could not recollect having been vaccinated, but having slept with it and handled the dead for weeks without taking it, was proof positive that he could not have it; but his case proved to be confluent small pox, and he narrowly escaped with life. If this subject had been vaccinated at the period when he was first exposed to small pox it is probable that it would not have taken, but it is equally probable that if he had had the matter properly applied at the time of the last exposure that it would have taken, and prevented the small pox.

It was remarked by the great discoverer—Jenner—that those who took the vaccine strongly were more liable to take the small pox afterwards than



those who took it lightly; showing, that in those who are very susceptible to the poison, a single application is not sufficient; but in all these cases a specific pustule will be produced from matter applied a second and, sometimes, a third time. The conclusion is, that a single application of the vaccine does not, in many cases, entirely destroy the susceptibility to small pox, but that a repetition of it will. The fact that the system is sometimes far more susceptible to the virus than at others, should warn us not to be contented, or consider a person positively safe, because the vaccine will not take when there is no epidemic influence; but if small pox is prevailing, and the vaccine will not take, if it is pure and properly applied, the subject is safe against even the slightest varioloid — at least this is my experience.

A great many cases are recorded as exceptions to the general law, tending to convey the impression to the public that the vaccine is not a preventive to all, and as every individual fears that he may be an exception, great fears are entertained whenever small pox makes its appearance. My rule is, when I vaccinate, to examine from time to time to determine if the disease is genuine, and if so, to repeat the vaccination at some subsequent period, being careful to have pure fresh matter, and to apply it carefully so as to ensure its success if possible. If it will not take a second time the subject is almost sure against even slight visitation. If, however, there is no epidemic influence at the time, the susceptibility may not be entirely destroyed, although the vaccine will not take. If, however, small pox is raging, and pure vaccine matter is applied and will not take, the small pox will not. I have never known an exception. To ensure safety, there is only necessary to vaccinate thoroughly and repeat it so long as it will take, and then, if small pox should become epidemic, vaccinate again, or whenever one happens to be exposed to it, and thus with very little trouble the human race can be rendered perfectly safe from this dreaded disease. I do not pretend to offer these observations as any thing new, but it is an undeniable fact that, from carelessness in vaccinating, small pox is becoming quite common, and vaccination is so common and severe during epidemic years as to call for some improvement. We have the remedy, and we have no excuse for not using it.



## *Adversaria Medica.*

By I. L. CRAWCOUR, M. D., *Visiting Physician, Charity Hospital.*

[Continued from last No.]

### II.

In my last I mentioned a case of irritable bladder depending upon a sacculated condition of the viscus. The pathology of this form of disease is but imperfectly known, but one of its causes is undoubtedly some obstruction to the passage of urine through the urethra. "In consequence of this the muscular coat of the bladder becomes highly developed, the fibres of the inner layer, especially, presenting reticulated appearance. No longer forming an even plane, these muscular bands become grouped together and pressed in the form of elevating decussating bars. The mucous membrane, insinuating itself into the interspaces, yields to the pressure, when the bladder contracts and becomes by degrees forced outwards, so as to form a pouch or bag."—(*Coulson on Disease of the Bladder and Prostate Gland.*) In consequence of this the bladder is never thoroughly emptied, the patient thinks he has urinated freely, but in the course of half an hour the same desire returns, and to his surprise he passes as much as on the previous occasion, and even after this, if a flexible catheter be introduced into the bladder, it will slip into the pouch and relieve it of the urine which, in consequence of the thinness of its muscular fibres, it is incapable of ejecting. With all this the urine does not necessarily become alkaline or phosphatic (although this is of frequent occurrence) because the fluid in the pouch is being constantly forced into the general cavity of the bladder, while fresh urine takes its place. The disease is probably in its essence incurable, but much may be done in the way of relief, as the treatment I adopted in the case cited proves.

I now pass to another case of irritable bladder, presenting almost the same symptoms as the last, but depending on entirely different causes.

Mr. S. R., aged 27, a resident of this city, called on me on the 20th of November, 1854, and complained of the following symptoms: Urinates every half hour, rarely can contain water over an hour, has a feeling of soreness over the whole of the lower abdominal region, the urethra is tender to the touch, and occasionally smarts while urine is passing through it, after urinating feels as if some urine were still left in the bladder and has to press on the perenium to expel the last drops; at stool, and especially if he strains, a discharge of glairy mucous frequently takes place from the

urethra—sometimes a drop, sometimes a tea spoonful is passed; is very dyspeptic; nervous; excitable and very hypochondriacal, costive; face very anæmic; pulse small—30; has been ill for years; married.

Examination of urine—color light red and containing a flocculent deposit, odor somewhat aromatic, sp. g. 1032, highly acid, boiled by hydrochloric acid becomes of a deep purple hue; on addition of nitric acid to a small quantity in a watch glass it becomes nearly solid from the formation of nitrate of urea. Examined microscopically—contained epithelial scales in excess, and large and numerous crystals of oxalate of lime, a few drops of muriatic acid added to an ounce of the urine produced an abundant precipitate of uric acid.

On examining the prostate, per rectum, it was engorged, soft and hot, pressure produced a feeling of pain and desire to urinate. Passed a No. 8 silver catheter into the bladder, it produced the most excruciating agony as it traversed the prostatic portion of the urethra; in other respects there was no other obstruction. Ordered—

R	Soda carbonatis.....	gr. x.
	Ext. conii.....	gr. v.
	Tinc. gentianæ.....	ʒi.
Ex.	Infus. gentiani ter in die.	
R	Hydrarg massa.....	} aa gr. x.
	Ext. aloes aquos.....	
	Camphoræ.....	gr. ii
	Ext. Hyoscyami.....	gr. xiii.

Ft. pil iv. one every night.

He was also ordered cold effusions and the use of the flesh brush night and morning.

November 29—has slightly improved, bladder not so irritable, appetite better, urine sp. g. 1025, no oxalate of lime, moderately acid.

In consequence of the engorgement of the prostate, I resolved to put him under the influence of iodine. Ordered—

R	Potassa iodini.....	gr. xxxvi.
	Liq. potassæ.....	ʒi. ss.
	Ext. conii.....	ʒi.
	Infus. buchu.....	ʒxvi. ℥

A wine glass full three times daily.

October 22—Can now retain water for six hours; very little pain in the perineum, and no mucous discharge after stool; omitted the liq. potassæ, and gave a dram of the syrup of iodide of iron in each dose of his mixture.

January 27—has continued to improve; holds water now steadily for three hours, but the passage of sound still causes great pain in the urethra, and especially in the prostatic portion. I resolved to cauterize.

February 3 —Cauterized the prostate portion of the urethra with Lallemand's porte costique, and continued treatment.

Feb. 18 —Now retains water four hours. (Cold weather.) Cauterized again.

Feb. 25 —Cauterized again; the application of the caustic produced very little pain.

March 18 —Averages four hours; cauterized again. Ordered—

R	Potassi iodidi.....	gr. xxiv.
	Syrupi ferri iodidi.....	℥i. ss.
	Iodinii.....	gr. vxiv.
	Syrupi simplicis.....	℥i. ss.
	Ol. anisi.....	℥i. ℥

Two tea spoonsful three times daily, after meals. Ex aquâ.

R	Ext. Belladonnæ.....	gr. vi.
	Morphiæ muriat.....	gr. ii.

Divide into 12 pills, one evening and morning.

March 29 —Only complains of a feeling of tenderness on the urethra about an inch from the meatus; passed a bougie armed with an ointment of nitrate of silver (arg. nit. ℥i., axung ℥i.) to the tender spot. Ordered a continuance of the medicines; the engorgement of the prostate has now disappeared.

May 3 —Is now well; rarely urinates more than three or four times daily, and is free from pain.

In this case the causes of the irritability of the bladder were many and complex, and any one would, of itself, be quite sufficient to set up disease. In the first place he suffered from the elimination of oxalate of lime, a most frequent cause of irritability of the bladder, in itself the result of imperfect assimilation of food; it not only interferes with general nutrition, but by its presence irritates the organ on which it is lodged. In addition to this there was the engorgement of the prostate, and a chronic inflammatory condition of the whole urethra, and especially of the membranous and prostatic portions. There was also the habit the bladder had acquired of expelling its contents on the reception of a small quantity of fluid, in itself a most annoying symptom, and frequently rendering the bladder less capacious than natural. In the treatment of oxalate of lime deposits I have generally found that attention to the functions of the skin, especially frictions with hair brushes, of essential service, and this, together with the long continued use of small doses of mercury and purgatives, and the better tonics has in more instances been sufficient to relieve the affection. This was the treatment I adopted in the present instance, at the same time en-



deavoring to lessen the extreme acidity of the urine and the mucous irritability of the patient by the administration of carbonate of soda and conium. Under this treatment the deposit disappeared as well as the excess of urea, evidenced by the diminution of the specific gravity of the urine; this latter symptom also showing that a marked improvement had taken place in the digestive and assimilative powers of the patient.

The next indication was the removal of the engorgement of the prostate; this had probably existed for some time, and I determined to try the influence of iodine. With regard to its efficiency most authors agree, and from its known power of controlling or removing other glandular enlargements, we should auger favorably of its action. I believe in many instances it fails solely from the insufficiency of the dose, or from the comparatively short time it is administered, and I am convinced that were it given more freely and were its use longer persevered in, we should much more frequently succeed. With regard to its use Mr. Coulson has the following remarks:

"Iodine has been strongly recommended, either given internally or applied in the form of ointment, to the enlarged structure. It may be ordered in the form of iodide of potassium, three grains three times a day. No speedy change can be expected, the medicine requiring a considerable time to effect any important diminution of the gland." In this view of treatment, Prof. Gross, of Louisville, concurs, and he adds: "In whatever form this remedy be used a long continuance of the article is demanded, care being taken to intermit its use for a few days every two or three weeks, as its good effects will be more likely to be elicited in this way, than when employed persistently." (*Gross on the Urinary Organs.*)

I consider the dose recommended by Coulson much too small to produce any effect; in large doses it almost invariably acts well, and I have never seen the absorption of the mamma and testes, and general marasmus so frequently mentioned by writers. Magendie states he has never known such a thing to occur, and Lugal observed that his patients fattened while under the influence of the remedy.

With regard to the third element of disease, in this case, it yielded readily to the action of nitrate of silver, and especial benefit was derived from passing a bougie smeared with the nitrate of silver ointment along the whole of the urethra. I have seen this patient within the last few days, and he tells me he has had no return of his distressing disease.



# Report to the Academy of Sciences of New Orleans, ON THE METEOROLOGY AND MORTALITY OF NEW ORLEANS, FOR THE YEAR 1854;

*Accompanied with a Chart illustrative of climatic influences in the production of disease.*

By E. H. BARTON, A. M., M. D.

The chairman of the committee on geography, statistics and meteorology, has the honor to report, that in relation to the last subject, viz : meteorology, he will embrace this opportunity of presenting to the Academy, the results of last year's observations. It is herewith comprehended in a table with every necessary detail that could be embraced in so small a compass. [*For table see end of this article.*]

The committee trusts not to overrate the value of these records. It is by them alone we can understand the climate in which we live, from which we derive our support, and upon which we are dependant for so many gratifications. The products whence flow our wealth, which clothe and feed us—the conditions, production of health, or insalubrity, are dependant upon and interpreted by a knowledge of records such as these.

As last year has been, in several respects, very remarkable, not only as it regards health, but agriculture, I will not merely leave you the table, but make some comparisons, and reflections deduced from the recorded facts, which may not be entirely destitute of value.

The year was as remarkable for the distribution of its climatic elements, as it has been for some of its extremes.

TEMPERATURE—In relation to this, *February* was two degrees cooler than the average.

I state it as a very unusual fact that *April* was cooler than *March*, by nearly three degrees!—was in every sense drier, and the mortality was consequently less by near 150.

*July* was two degrees warmer than the average. And the extremes in the latter part of *June* and in *July*, were almost unprecedented for their high extremes: 95 deg. was reached several times—a very remarkable height here, in a fair exposure.

Again, *September* was two degrees warmer than the average.

*November* was three degrees cooler.

*December* more than six degrees cooler than the average—tending to make the entire year about one and a half degrees cooler than the average.

SOLAR RADIATION was also higher than usual, as in every sickly year.

In relation to the DEW POINT—During *March* it was nearly ten degrees higher than the average, and during *April* seven degrees less—again accounting for their great difference in salubrity.

During *June* about five degrees less.

During *July* about half a degree less.

During *September* near two degrees higher—our most sickly month last year.

During *October* two and a half degrees less.

During *November* seven degrees less.

And during *December* two degrees less than the average.

With regard to the BAROMETER—During the first six months it was unusually low, and during the last six, unusually high.

HUMIDITY—During *February* and *March* the moisture was much over the averages.

During *September* over the average, and during the balance of the year the reverse, and to a very remarkable degree.

RAIN—The *precipitation* during the entire year was more than *one inch* over the average of seventeen years. But the most remarkable circumstance was in its distribution.

During *January* there fell less than during seventeen years back, although the very reverse took place during *February*—being more than any preceding February, except that of 1842, and being near ten inches.

During *August* the precipitation was much under the average, being not much more than half the average.

But *September* was the heaviest in my record since 1834, and there resulted, what usually follows with the wettest month, viz.: most sickness.

During *November* and *December* there fell little more than one-third of the usual quantity for seventeen years, and to make the record more complete, embracing the winter just past (but little out of the record) there has fallen during *January* and *February* of the present year, not more than twenty-eight per cent. of the average of seventeen years.

WINDS—There has been a great preponderance during the year of easterly winds, as is usual during epidemic years; those from the west have been deficient—they prevail during wet years. The calms have been unusually numerous.

Now if that record is of any value, it is derived from applying it to practical purposes. Its true interpretation then is its influence over

the great industrial pursuits of life—upon the products which sustain and clothe man, and upon his sanitary condition.

Want of time will compel me to be very brief.

The heavy rains of September, about the period of the gathering of our great staple—cotton—materially impaired its productiveness, this was in a great measure compensated by the fine dry picking season of the balance of the year, frosty weather having been retarded until after the first week in November.

The cane requires for its growth and maturation much moisture in June and July—these are usually our most rainy months. Last year these months were defective in precipitation, the cane suffered accordingly. When, on the contrary, the cane is ripening, too much rain injures its saccharine qualities; the heavy rains of September which are so different from what is customary here, tended materially to shorten the crop of the year, which otherwise would have been very great.

The remarkable drought which has now continued in such an unprecedented degree since October, it is said, has injured the cane in the ground, subjecting it to the dry rot. This is hardly credible. In the low alluvial grounds of the Mississippi, the water is not permanently far from the surface, and at no period has this varied, from experiments made by myself, over eight or ten inches, during the entire year. This we know depends upon the surface water, (from rains,) and not upon the more or less height of the river. These experiments have been made in my garden, upon land that has been artificially elevated, as most of the city is; the water in my well, dug and prepared for the purpose, varied from twenty-one or twenty-two inches from the surface to thirty-one or thirty-two

Planters and farmers are proverbially hard to please in relation to the weather; it is rarely exactly right for them long, being too wet or too dry, too hot or too cold, or the spells continuing too long. Now it is possible that the cane out of the ground—in *matelat* (as it is called) may be somewhat injured by too much desiccation, but when properly put up and covered, I suspect the extent of it has been much exaggerated. Time alone will show.

But, in another point of view, all of us have sustained great injury. The wheels of commerce have been almost arrested, because the products of the husbandman cannot be brought to market, to supply the wants of the community. Commerce, the life blood of society, which is the great agent performing for States what the heart does in the circulation of the blood



to the body, is stagnant; because the windows of Heaven have been so long closed, and sufficient rains have not fallen to raise the streams which tend directly and indirectly to produce and increase prosperity, exchanging the products of industry and supplying the vast and multiplied wants of the community.

For the first there is scarce any remedy, except in the industry and enterprise of society in providing other means of communication with the great productive basins North and West, and if we call it "king," it exhibits its predominant influence on our comforts and on our wants.

But there are other circumstances in relation to the climatic influence, which it is not proper entirely to overlook in this connection, these relate to its bearing on our sanitary condition. Upon this subject I am compelled to be very brief, against my inclinations, for, as astonishing and incredible as it may appear to many people abroad, this city has no Board of Health or record through which to obtain the necessary facts to show the full influence of these unusual conditions on health, and what facts I have been able to procure have been at great expense of labor and time.

This year has furnished a running commentary and strong corroboratory testimony of all the experience I have so often given in relation to the influence of humidity, and its reverse, on health in this climate. The moist months have been the sickly months and the dry months have been the healthy months, (in general,) and I am sure with the exception of cholera, which is usually produced when there is a great variation in the drying power, no winter in Louisiana has been more healthy and delightful than that which has just passed.

Most of you will probably be surprised to learn that the mortality in this city last year was upwards of 10,800; that of this there died of yellow fever more than 2400, and by cholera, during the last seven months of it, near 900! The largest mortality by yellow fever occurred, last year, in September, according to a general fact, that it usually takes place during that month in which there is the largest precipitation and highest solar radiation, (as see chart,) and particularly the latter.

The conditions productive of cholera are different from those giving rise to yellow fever. Neither the temperature, moisture or radiation required to produce the latter, is required for that. It is rather a great variation of these three elements within certain ranges, but more especially variations of the drying power. This is made very conspicuous by reducing the mortality in the form of a chart, from the ascent of the line indicating it—



the larger part occurring during the remarkably dry month of December. Precisely the same occurred all along the northern boundaries of the United States last summer, during their unprecedented drought, in exact accordance with these views, as shown to me by my very intelligent correspondent, Prof. Hunt, of the University of Buffalo, N. Y.

Upon other occasions I have endeavored to show the effect of humidity on health, and to show how dangerous the combination is with filth and heat. The present year furnishes me ample means of corroborating and illustrating my positions.

It has been said before, that the summer was unusually hot, reaching extremes of temperature we have rarely had to endure here. This was marked by the effects, in concurrence with a nearly saturated atmosphere, by the occurrence of more cases of *coup de soleil*, as it is miscalled, than has ever taken place in this country within my knowledge. I have taken detailed notice of this in another place,\* and therefore shall only simply allude to it here. It is not then *sun stroke*, for the temperature in the direct rays of the sun, is often from 10° to 20° or even 40° higher without producing this effect. It arises, according to the record I have shown, during the highest temperature in the shade, and a nearly saturated atmosphere, on individuals who have made themselves susceptible through intemperance. About two weeks after the delivery of that address I received from Prof. Charles A. Lee, of New York, a letter from which, with the permission of the Academy, I will read an extract: "You recollect that a year ago last August, there were more than 100 deaths in one day in New York by what was erroneously called 'sun stroke,' the dew point nearly corresponded with the temperature." Other parts of that remarkable letter will show that I have had the very distinguished honor of anticipating that eminent man on other parts of this subject, or rather of putting his preconceived theoretical views into practical application.

Here then, are three of the most fatal maladies to which our race is subject, whose etiology is already traced to specific climatic conditions. But, as these considerations absolutely, and most fortunately for us, require for the production of their lethiferous influence the concurrence of an *adjunct*, it is nearly always in our power, by *preventing this union*, to control the result! For the development of the first two, (yellow fever and cholera,) filth or malaria is indispensable — no concentration of heat and humidity combined will produce yellow fever — no variation of these with the drying

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\* Address before the State Medical Society.

power will produce cholera; and I am almost equally sure that heat and humidity alone, without the strong predisponent mentioned, will ever result in the third fatal malady. Hence then the highly gratifying conviction that man has control over all!

But the exposition of these climatic developments does not stop here. Humidity in conjunction with heat is essential to the evolvment of all the higher grades of fever, and when filth or malaria is present, fever will *certainly* break out. We can now understand in the language of my distinguished correspondent just quoted,\* why it was that Capt. Cook preserved the health of his entire crew during his long voyage around the world without losing a man; it was by dry rubbing the decks and the use of the holy stone, (as it is called at sea,) and keeping his ship clean and not by constantly washing the decks, not only those above where they can soon be dried by sun and wind, but below, where the ventilation and drying power is very limited. Such is a constant effect of this mode of cleanliness as expressed to me by some of the first officers of our navy.

It is a custom, and a highly beneficial one too, in some of the sickliest parts of many southern countries to kindle fires night and morning in private dwellings during the sickly season, and I know from much experience, that it is highly conservative of health, and many a time have I ordered them to be made in the sick room. It was an empirical process which was known to do good without our understanding the rationale—or the principle on which it was based, to explain its beneficial effects. “The discovery of a principle,” it is truly said, “is the discovery of a truth;” \* and its benefit is only limited by the extent to which it is applicable.

During the late epidemic yellow fever, business took me up the coast; on returning I had to take a St. Louis steamboat, loaded with stock, and so offensive as to be almost beyond endurance. I remarked to my companion, (a professional gentleman,) that there would probably ensue a large mortality from on board the boat when she reached the city. In a few hours all this humid effluvia subsided—walk to any part of the boat, it could no longer be perceived; a great change in the weather had occurred. I sought a thermometer to take the dew point, but there was none on board; in a couple of hours we reached the city, and I immediately took the dew point, and found it at the degree where I had before announced, † that it was incompatible with the continued existence of the epidemic yellow

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\* Prof. Lee, in a letter to the author.

† See the Sanitary Report.







fever—that is about  $60^{\circ}$ ; accordingly, from that time the fever abated.

So, humidity is essential to the existence of *all odors*—whether the fragrance of the rose or the stinks of sewers, and the morning breeze is either replenished and enriched with the odors of flowers, the bounteous gifts of nature, or is poisoned with the offals of man's filthiness and negligence, as the moisture or dryness predominates.

From the interpretation resulting from the establishment of this principle, we shall no longer be at a loss to account for the occurrence of the fevers incident not only to ships, but jails, penitentiaries, hospitals and all filthy crowded places; and can we not be enabled to thus account for the general exemption of jails and the upper stories of houses when the inmates are isolated there, in this and other countries (where it is usual to lock one's self up from the plague.) during the prevalence of malignant fevers, as occurred here in 1853, when the health of the jails was effected for the first time?

The character of the fever will, in a great measure, depend upon the degree of temperature and humidity, the amount of filth, and the susceptibility of those exposed; from the Egyptian plague and yellow fever to typhus, gravior and mitior and bilious fever.

Indeed there is too much reason to believe that all classes of fevers depend upon the varying amount of humidity and temperature, as essential requirements, and if we look at the characteristic condition of opposite seasons in different climates and their effect on salubrity, we shall find this view of it strongly sustained, and they are only varied by physiological peculiarities and individual liabilities.

Can we say where this important principle is limited in interpretation? or to what it does not furnish a key? Is it saying too much then, that that little instrument (the hygrometer) will be as sure an index of safety and salubrity, in the causes productive of malarious fevers, as Sir Humphrey Davy's safety lamp is to the miner buried in the bowels of the earth?



## Yellow Fever:

THE MANNER OF ITS RISE AND PROGRESS IN THE CITY OF NEW ORLEANS.

By. E. D. FENNER, M. D.

*Messrs. Editors*—In compliance with your request to contribute something for your Journal, I shall endeavor from time to time, as leisure per-

mits, to notice briefly and practically some of the more prominent and important points in the nature and management of yellow fever. For the last thirteen summers I have devoted special attention to the study of this disease, our city's greatest foe, and have not withheld from the profession and the community the results of my experience. Whatever conclusions I have formed on different points have only been arrived at after the most careful consideration of the largest number of facts I could collect together, and a full statement of these facts has always allowed my readers a fair opportunity to draw their own inferences. I have had the gratification to find my positions supported by some of the clearest men of the day, whilst others have either denied them *in toto* or passed them by without examination. Now, I do not claim for a moment to be the *first* to take the positions I maintain — I only say they are *my own conclusions*, derived from all the facts that have fallen under my own observation.

On this occasion I can only call attention to *one* of these positions, and I do so now, that the physicians of New Orleans may either confirm or refute it according to the facts that are at this moment transpiring before their eyes.

It relates to *the mode of origin and the manner in which yellow fever annually commences its ravages and runs its course in this city.*

I maintain that this disease never *bursts out suddenly* here, presenting characteristic features different from those of all other fevers, and therefore, easily distinguishable by the experimental physician; but, on the contrary, that the endemic fevers (bilious, remittent and intermittent) gradually run into the yellow fever type, assuming its features in such a manner as to excite *suspicion*, but yet not to justify a positive diagnosis, until one or more cases have been observed to terminate in black vomit or some other hemorrhage. Hence arise the discrepancies of opinion so often displayed by physicians as to the true character of certain cases that are seen at the beginning of an outbreak of yellow fever. At this period may be seen numerous cases of bilious remittent fever, presenting more or less the aspect of yellow fever, but not in so strong a light as to remove all doubt from the diagnosis.

As the season advances, if it is going to be a *sickly one*, the yellow fever type will become better marked and the number of cases will increase until it predominates over all others; then, if the disease prevails to a great extent, it is called an *epidemic*. This rages for a period usually, here, of sixty or seventy days, when it declines gradually and is again merged into the ordinary types of endemic fever.

But if the season turns out to be *not very sickly*, the yellow type will not gain the ascendancy over all others, but only appears *sporadically* and to a moderate extent. On these occasions we have plenty of intermittent and remittent fever, and some called *dengue*, which approach nearest the yellow fever type, but few cases are marked yellow fever unless they present unquestionable signs.

I have repeatedly demonstrated the correctness of these observations by statistics made out from the records of the Charity Hospital and the reports from our city cemeteries. \* And I appeal to you, Messrs. Editors, who are in daily attendance at the Charity Hospital, to say whether they are not verified by the occurrences presented at the present moment. For the last two weeks I have pointed out to the resident students of the hospital and such physicians as have visited my wards, the fact that the endemic fevers of the day showed a decided tendency to run into the yellow fever type.

On the 19th inst. a plain case of yellow fever was admitted into the hospital and died of black vomit on the morning of the 20th. Since that time there have been three other deaths with black vomit, and new cases are now admitted every day. So far the disease is unquestionably of *domestic origin*; not a single case as yet having been traced, by the remotest probability, to *foreign infection*. The hospital cases are brought in from different and remote parts of the city, showing no sort of communication whatever. The question of the foreign or domestic origin of the yellow fever is of vast importance to the citizens of New Orleans, as it must form the basis of all rational measures directed to the prevention or eradication of the evil.

If yellow fever is a foreign disease, imported from abroad, it surely may be kept off by a rigid Quarantine, but if it originates at home, what hope of its removal is left us but to discover and destroy its local causes?

The examination of this question may form the topic of a future communication, and I shall, therefore, dismiss it for the present. What I have said of the manner in which yellow fever begins and progresses in New Orleans, is equally true in general at the various places where this disease has prevailed in this region, as is shown by the testimony I have collected on the subject within the last five years.

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\* See *Statistics of Fever* in the New Orleans Medical and Surgical Journal, 1846-7, and Southern Medical Reports.

## Editorial and Miscellaneous.

### THE HEALTH OF OUR CITY.

We propose to commence in this number of our Journal a series of reports relative to the sanitary condition of our city, together with the list of mortality for each month, ending on the 25th.

To tell the truth is said to be sometimes not a very agreeable duty — as, for instance, when it conflicts with our immediate personal comforts or pecuniary welfare; yet it is an acknowledged and irresistible axiom, that the *ultimate* advantages which attach to the declaration of the plain, unvarnished truth, are of far greater import, and, consequently, entitled to our deepest consideration. In everything that pertains to medicine, truth is eminently indispensable, and, as public journalists, we feel it doubly incumbent on us to write down her promptings, let them be for weal or for woe. It is as much our duty to warn a friend of danger which we believe to threaten him, and which he may possibly avert, as to offer to extricate him after he has fallen into it. We do not aspire to the position of alarmists, yet we do not shrink from the risk we incur of having this appellation applied to us by the selfish or the misguided, so long as we have the truth on our side.

How could we reconcile it to ourselves to declare New Orleans at the present time in a cleanly condition, when we have the most unmistakable evidence to the contrary constantly before our eyes, and labor under the honest conviction that there is readily removable filth enough all over the place to engender any species of pestilence whatever? Nor can we degrade our pages with false declarations to the effect that our city is, or has, within the past month, been healthy, when unerring figures prove the contrary beyond all cavil. It is not our mission, nor is it our purpose, as editors of a medical journal which we shall exert ourselves to make useful, not only to the profession but to the public, to write *ad captandum* articles for the promotion of this, that, or the other selfish clique, painting in *couleur de rose* the hideous deformities of our sanitary system, or insulting the common sense of every right thinking man with nauseous assertions that hygeia reigns supreme, when they see death stalking ruthlessly over her; we leave such work to those who seek the contracts, and, without stint, we extend them our heartfelt pity. Our aim is advancement in medical knowledge, in all its attributes, and the mitigation of human suffering, not only through the means of cure, but through those of prevention; and we believe this to be attainable only through the medium of truths recorded for digestion.

About the 15th of May last, and when quite a fuss was being made in certain quarters about the salubrity of our climate — the mortality of the week, ending on the 19th being in all 136 — cholera made its appearance in the Charity Hospital; for a day or two it excited no special attention, as our physicians have been ac-



customed to seeing sporadic cases of the disease very commonly since the great epidemic of 1848-9. Soon, however, it became evident that the disease was prevalent, and, although we find no published returns of deaths by cholera separately, we notice the total week's mortality of our city for the week ending on the 29th of May, advanced from 136 to 177, and this without returns from St. Patrick's cemetery, which is well known to be one of the well patronized cemeteries of our city. Allowing the difference between 136 and 177 (41) to be so many deaths from cholera, and adding twenty more deaths by the disease from St. Patrick's cemetery, and we have sixty-one deaths by cholera for the first week of its prevalence—indeed, we have it from reliable authority, that there were really sixty-seven. The disease now increased steadily, as was to be seen, not only by those who frequent the hospital, but by those only engaged in private practice, and the published list of mortality for the week ending on the 28th of May, shows a sum total of 381 deaths, 202 of which were from cholera. Things continued to grow worse until on Monday, June 4th, our community was furnished with a published list of mortality showing a total of 504 (!) deaths, 278 of which were from cholera. During the latter days of this fatal week, a decline was noticed in the epidemic, and the next published list of mortality, for the week ending June 10th, showed a total of 381 deaths, 201 of which were from cholera. From this time the epidemic subsided rapidly, and for the week ending on June 17th, we have a published list of 236 deaths, seventy-six of which are from cholera. Finally, for the week ending June 24th, we have a published list of mortality showing a total of 191, deaths, forty-eight of which are from cholera.

To the reader of the foregoing statistics it is supererogation to say that New Orleans has not been, during the past five weeks, in a healthy condition; nor is it our province to stop here and inflict on those who so well deserve it, the castigation they merit at the hands of all honest men, for reiterated assertions that New Orleans was *healthy*, while, in addition to what we shall demonstrate to be a large mortality from other diseases, she was losing by cholera, an average of nearly twenty-five of her citizens daily, for five weeks. Nor can we do more in this place than simply call the attention of all right thinking men to the lamentable fact that by these same highly interested and self-constituted guardians of the public health, our Board of Health was sneered at and almost censured, for declaring the disease epidemic, and for warning the people accordingly. It is enough to know now, that the figures (*facts*) most fully sustain them in the act, and insure them in future all that confidence from the community which their neighbors seem to have coveted.

This epidemic of cholera has been particularly severe amongst negroes and little children; it attacked numbers of the latter, amongst high and low, with extreme malignity; little infants from one to three months being subjected to all those distressing symptoms, vomiting and purging of rice water, cramps, etc., and dying within a few hours of the first accession of symptoms. As a rule, however, the patients had plain premonitory symptoms, which could almost always be checked if at once attended to. On the heads of the anti-cholera advocates must rest many of the deaths we have now to record.

We have said above that, independent of the deaths by cholera, (in all 866,) New Orleans has not been in a healthy condition. We resort again to figures to prove the position.

The census of 1850 shows the cities of Boston and New Orleans to be *about* the same in point of population—though, if it be required, we will give our city the benefit of ten or fifteen thousand more. We claim in the winter a “floating population” here, which swells the list considerably, but it will be generally conceded that this class of people were not here during the past five weeks. Now let us compare the health of the two cities as evinced by their weekly reports of mortality.

For the week ending May 13th, the deaths in New Orleans were 136; for the week ending May 12th, the deaths in Boston were 78—difference against New Orleans, 58. For the week ending May 20th, deaths in New Orleans, exclusive of cholera, were 136; for the week ending May 10th, the deaths in Boston were 62—difference against New Orleans, 74. For the week ending May 27th, the deaths in New Orleans, exclusive of cholera, were 179; for the week ending May 26th, the deaths in Boston were 61—difference against New Orleans, 118. For the week ending June 3d, the deaths in New Orleans, exclusive of cholera, were 226; for the week ending June 2d, the deaths in the city of Boston were 73—difference against New Orleans, 153. For the week ending June 10th, the deaths in New Orleans, exclusive of cholera, were 180; for the week ending June 9th, the deaths in Boston were 64—difference against New Orleans, 116. Total of deaths in New Orleans, exclusive of cholera, for five weeks, ending on June 10th, 857; total of deaths for Boston, for five weeks, ending June 9th, 338—difference against New Orleans, 519. Add the number of deaths, exclusive of cholera, to those caused by cholera up to June 10th, 742, and we have a total mortality in New Orleans, for five weeks, of 1599.

Far from us be the intention of doing or saying aught, through evil motives, to injure the community we live in, the community of our choice; but when we see evil influences so brought to bear on our people as almost to stultify them, to completely blind them to their own best interests, the most powerful truths, however disagreeable, are the only weapons which can be efficiently used in their defence. As individual members of the community, having families here to care for, we feel that we have the right, as well as the will, to use every honorable effort to bring about a change in affairs. We can never expect to effectually quarantine ourselves against disease, while we are every day, by our neglect, building hot beds for its propagation here amongst us.

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**EXTIRPATION OF THE UTERUS.**—Professor Geddings has recently extirpated an inverted uterus with entire success. The organ appears to have been protruded entirely without the vulva. A ligature was thrown around the vagina, above the tumor, and then excision was performed.

## AMERICAN MEDICAL ASSOCIATION.

The late meeting of the American Medical Association has been a truly brilliant one. Notwithstanding the fact that little in the way of reform has yet been effected by this body, we are forced to believe that the day is not far distant, when its influence will be felt throughout the length and breadth of the land. Great and enduring reformation of any kind should have a gradual growth. Moreover, in a republican country like ours, there is no one man, or set of men, whose word is law; great measures of reform are to be sanctioned by the masses, and more time is required to effect important changes, more especially where they have to be effected independently of legislative enactment. As heartily as we might desire true, radical reform in everything that pertains to medicine, and however sincerely we may believe that it cannot come one moment too soon, still, there is no cause for despondency on account of the limited results thus far achieved. It is with unfeigned regret that we have noticed decided sneers against the American Medical Association by members of our profession whose positions give us a right to expect better things of them; it is very easy to sneer, and if they desire to injure the great cause of medical reform, they could not choose a better weapon. But how can they reconcile it to themselves that they have thus rendered their mite towards the attainment of the reform which they, themselves, promptly acknowledge to be indispensable?

The American Medical Association has been in existence but eight years; the tree has hardly had time to take firm root as yet, much less is its young body able to stand erect against the elements with which it is necessarily brought in contact: it is not yet well pruned; indeed, it has been allowed to become rather top-heavy; hence the bending and swaying we have witnessed when it has been subjected to the rude blasts of the medical schools, etc. But the seed was good, and the soil rich, and although we may not live to enjoy the realization, the day must come, when a leaf plucked from this great tree will be the indispensable diploma of the medical man of this country. The Association has everything to contend against, from the combined resistance of the medical schools—from which we have a right to expect better evidences of enlightenment—down to the lowest sectional prejudices of the most ignorant of our order. Yet, the experiment gives fair promise of success; the few meetings which have been held have started the development of minds which must otherwise have remained in comparative obscurity. In twice eight more years the struggle will be to become the distinguished member of the American Medical Association.

It is with feelings of surprise and humiliation that we look in vain for the name of a delegate from Louisiana to the late meeting in Philadelphia. Would that it were not our duty to record the fact. Truly is medicine at a discount in New Orleans, where, from her scores of time honored, rich and learned doctors, not even one could be found to represent us in our great national council. May we never again be called on to record such a delinquency.

The hospitality extended to the delegates by our Philadelphia brethren, seems to have been of the most elevated, appropriate and agreeable character. They have set an example which, it is to be hoped, will be imitated in future, and thus forever



do away with the enactment of scenes which can only tend to degrade us in the eyes of our fellow men.

We cannot close without appending the following gem from the pen of our "physician poet," Dr. O. W. Holmes, which was circulated amongst the delegates, and has since been going the rounds of the medical press:

A TOAST—*By Oliver Wendell Holmes, M. D.*

A triple health to Friendship, Science, Art,  
From heads and hands that own a common heart!  
Each in its turn the others' willing slave;  
Each in its season strong to heal and save.

Friendship's blind service, in the hour of need,  
Wipes the pale face — and lets the victim bleed.  
Science must stop to reason and explain;  
ART claps his finger on the streaming vein.

But Art's brief memory fails the hand at last;  
Then SCIENCE lifts the flambeau of the past.  
When both their equal impotence deplore, —  
When Learning sighs, and Skill can do no more,  
The tear of FRIENDSHIP pours its heavenly balm,  
And soothes the pang no anodyne may calm!

MAY 1st, 1855.

DISCUSSION AT THE IMPERIAL ACADEMY OF MEDICINE ON M. DEPAUL'S REPORT ON THE TREATMENT OF DISPLACEMENTS OF THE UTERUS BY INTRA-UTERINE PESSARIES.—In the *Monthly Journal of the Medical Sciences* is quite a full report of the recent important "discussion" of the above very interesting subject. As the matter stands, a principle, either *pro* or *con*, is far from being established. M. Depaul's report is adverse to the use of Intra-uterine passaries; he stigmatizes the treatment as "repugnant to common sense," and we find him supported by MM. Hervez de Chégoin, Cazeaux, Amusat and Malgaigne; while MM. Huguier, P. Dubois, Velpeau, Ricord and Robert advocate the use of the instruments more or less strenuously.

The Academy finally made a sort of compromise of the matter, by passing the following resolutions.

1. The cases reported to the Academy by MM. Broca and Cruveillier, along with numerous others on record, prove that the use of the intra-uterine pessary may occasion serious accidents, and, sometimes, even death.
2. In the rare cases in which this instrument has appeared productive of advantageous results, it is not proved that it had always acted by replacing the uterus.

CASES OF POLYPUS OF THE WOMB—*By Walter Channing, M. D.*—We are indebted to the author for a copy of his valuable pamphlet bearing the above title. A careful perusal of his papers, as they appeared in the *Boston Med. and Surg. Journal* for March 8th and 15th, 1855, afforded us much pleasure and instruction, but we are much obliged to Dr. Channing for placing before us the valuable fruits of his labors in a more tangible form.



THE WEEKLY SOUTH CAROLINIAN.—We take pleasure in acknowledging the receipt of this valuable journal, published at Columbia, South Carolina, by Messrs Gibbs & Johnston, and shall always welcome it amongst our exchanges. We can assure its editors, that it is not the less acceptable for not belonging to the medical press. We shall be pleased to extend our South Carolina acquaintance.

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TO OUR SUBSCRIBERS.—We regret to learn through our collector, who has recently been paying his respects to our city subscribers, that our carrier has failed to deliver some numbers of the *Gazette*. We trust that our patrons will believe that we make every effort to have them served with their numbers promptly and regularly, and we will take it as a special favor if they will promptly notify us of any future delinquency of the carrier. It is only through the subscribers themselves, that we can procure this information, and a few words from them by note will always set matters right. Our editions are full, and we are prepared to replace any lost numbers immediately.

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#### CHARITY HOSPITAL.

Since our last issue there have been few cases of interest in the Surgical wards. A case of chronic scrofulous synovitis may be mentioned, which necessitated an amputation of the thigh. This affection of the joint was of long standing, and was gradually wearing the patient out. The affected joint, developed to three or four times its natural size, presented to the touch a soft and elastic tumor, unaccompanied by superficial excitement, but simulating true fluctuation very closely. After the amputation the muscles and arteries were found much altered in texture, showing that the nutrition of the limb had been much impaired. The alteration of the muscles consisted in an advanced fatty degeneration, while the external and middle coats of the femoral and other arteries, were studded with calcareous deposits. Owing to this diseased condition of the arteries, fears were entertained of secondary hemorrhage; this, however, did not occur, and the stump healed in nearly all its points by first intention.

On opening the joint, the different textures were found completely disorganized. The tumor seemed to be composed of a brown fibro-gelatinous mass, containing here and there a few cells or cysts, which were filled with a dark bloody serum. The heads of the bones seems to have undergone a similar degeneration, and the tibia and fibula throughout their whole length seemed to be devoid of all earthy matter, and presented the appearance of cartilage.

An operation was also performed for the relief of strangulated inguinal hernia of three days standing.

The portion of intestine contained in the sac was found to be gangrenous. The patient died forty-eight hours afterwards of peritonitis.

Amputation of the fore-arm, necessitated by caries of the bones of the wrist, was performed, but the patient, worn down by hectic and suppuration, died a few days afterwards.

In our last number we reported one case of yellow fever as having occurred; since then we have seen several others, the history of which we subjoin:

Admitted,	-	-	-	-	-	-	-	984
Discharged,	-	-	-	-	-	-	-	875
Died,	-	-	-	-	-	-	-	145
Births.—females 3. 1 still-born.								

## Excerpta.

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AMERICAN MEDICAL ASSOCIATION — *Eighth Annual Meeting, held at Philadelphia, May, 1855.*— The Association met in the Musical Fund Hall, in Locust street, on the 1st inst., when they were welcomed by Dr. Isaac Hays, of Philadelphia, in behalf of the Committee of Arrangements and Reception.

The President, Dr. C. A. Pope, of Missouri, was in the Chair.

The whole number of names registered during the session was 530. There were from the State of New York, 80, of which 44 were from the city. All the States were represented, except two or three, and likewise the United States Army and Navy, and the District of Columbia, with the American Medical Society of Paris.

The Association having been duly organized, the President delivered the annual address, which was listened to with marked attention, and a copy requested for publication in the Transactions.

On the recommendation of the Committee of Arrangements, the sessions were ordered to be held from 9, A. M., to 3, P. M., with a recess from 12 to 1 o'clock.

The delegates from the several States then met together, and appointed one of their number to compose a

*Nominating Committee.*— The following gentlemen were selected: Maine, A. J. Fuller; New Hampshire, Silas Cummings; Vermont, Israel Hinckley; Massachusetts, C. P. Fiske; Rhode Island, Jos. Maura; Connecticut, P. A. Jewett; New York, John McCall; Pennsylvania, J. B. Biddle; New Jersey, Louis Condict; Delaware, James W. Thompson; Maryland, Charles Magill; District of Columbia, Thomas Miller; Virginia, B. R. Welford; North Carolina, O. F. Manson; South Carolina, P. C. Gaillard; Georgia, Richard D. Arnold; Alabama, P. H. Cabell; Tennessee, J. Berrien Lindsley; Kentucky, C. J. Blackburne; Ohio, R. S. Hills; Indiana, Joel Pennington; Illinois, J. V. Z. Blaney; Michigan, A. B. Palmer; Mississippi, L. P. Perry; Iowa, J. E. Sandbourne; Wisconsin, J. B. Dousman.

An animated discussion then ensued as to the propriety of taxing permanent members, in which Drs. Condie, of Philadelphia, Watson, of New York, and others, took part. On motion of Dr. J. White, of New York, the subject was referred to a committee of three; this committee subsequently reported the following resolutions, which were adopted:

*Resolved*, That upon no permanent member who is not present at a meeting of the Association, shall be assessed the annual contribution; but no one shall be entitled to receive a copy of the printed Transactions, unless he pay into the treasury a sum not less than the annual assessment paid by the delegates and permanent members in attendance; and that all the names of permanent members in attendance, and all the names of permanent members that have been left off the published list, be inserted in the next volume of Transactions.

*Resolved*, That no assessment whatever be made against members by invitation, but that they also be entitled to a copy of the printed Transactions, by paying the sum assessed upon delegates in attendance.

*Committee on Prize Essays.*— Dr. Alfred Stillé, of Philadelphia, presented for Dr. La Roche, the Chairman of the Committee on Prize Essays, the report of that Committee. The Committee had received six essays, all of which had been carefully examined. One of them was found to be pre-eminently entitled to a prize; its subject was, "Statistics of Placenta Prævia." The sealed note containing the same motto as that on the essay was then opened, and the author was found to be Dr. James T. Trask, of Whiteplains, Westchester county, New York. The name of the successful author was received by the convention in a highly complimentary manner.

Dr. Lemoine, of St. Louis, read an extract of a report upon "Epidemics of Missouri, Illinois, Iowa and Wisconsin," which was sent to the Publication Committee.



Dr. Stewart, of New York, moved that the Nominating Committee be instructed to present the names of three candidates for the office of President, to be balloted for by the Association. After considerable discussion and reference to the constitution, the motion was laid on the table.

Dr. Sanford B. Hunt, of Buffalo, N. Y., from the Committee on the Hygrometrical State of the Atmosphere in various localities, and its influence on health, submitted a report; after the reading of which the Association adjourned.

#### SECOND DAY.

The Committee of Arrangements reported the names of those delegates who had arrived during the recess.

On motion of Dr. Condie, of Pa., the Secretaries were directed to afford every facility to the reporters of the press.

Dr. J. L. Atlee, of Pa., made a report on the subject of the block which had been ordered by the Association to be prepared for the national monument at Washington. The work had been performed, and the stone was then on exhibition in one of the committee rooms. The idea of its design had been suggested to him by the late Dr. Pierson, of Massachusetts, who was killed by the railway accident at Norwalk, in 1853. It was executed by Mr. Beck, a young American artist. The marble was obtained from the State of Vermont, and is of a fine texture, and beautifully white. It is about four feet long and two feet deep, and comprises five full length figures in relief—Hippocrates indignantly refusing the presents of Artaxerxes, who was striving to obtain his services, and replying to the Persian emissaries, "Tell your master I am rich enough; honor will not permit me to serve the enemies of Greece." This beautiful work of art was much admired, and the Association appropriated one thousand dollars to pay for it.

*Officers for 1855.*—Dr. J. B. Biddle, of Penn., Chairman of the Committee on Nominations, now reported the names of the following gentlemen as officers for the present year:

*President*—Geo. B. Wood, of Philadelphia.

*Vice President*—William M. Boling, of Alabama; Daniel Tilden, of Ohio; D. Humphrey Storer, of Massachusetts; Grafton Tyler, District of Columbia.

*Secretaries*—Francis West of Philadelphia; R. C. Foster, of Tennessee.

*Treasurer*—Caspar Wister, Pennsylvania.

On motion of Prof. Rogers, of Philadelphia, the nominations were confirmed; and the President elect being conducted to the chair by a special committee, returned thanks.

The Committee on Nominations also recommended, but not unanimously, that the next meeting should be held at Nashville, Tenn. Various places were named as substitutes for Nashville, and finally the Convention selected Detroit, and changed the Secretary—appointing Dr. Wm. Brodie in place of Dr. Foster, who resigned.

Dr. S. B. Hunt's report was concluded, and referred to the Committee on Publication.

A committee was ordered to be appointed to endeavor to procure commutation tickets for members attending the meetings of the Association; and thanks were returned to the Philadelphia, Wilmington and Baltimore Railroad Company, for courtesies extended by that Company.

Dr. Frank H. Hamilton, of N. Y., read an interesting report "On the Frequency of Deformities in Fractures." Referred to be published.

Dr. Charles Hooper, of Connecticut, presented and read a report on the subject of "Diet for the Sick." Ordered to Committee on Publication.

The hour of recess having arrived, the members proceeded in procession, each bearing a distinguishing badge, to accept an invitation from the Mayor of the city to visit Independence Hall. On arriving, they were collectively presented to Mayor Conrad by the Chairman of the Committee of Arrangements, in an appropriate address, to which the Mayor replied at length, welcoming the members to Philadelphia, and assuring them of the great interest that his fellow citizens took in their proceedings.



On re-assembling after the recess, a vote of thanks was passed to Mayor Conrad for his cordial reception, and a copy of his address was requested for publication in the Transactions.

Dr. Thompson, of Delaware, submitted the following resolutions, which were made the special order for ten o'clock to-morrow. They were fully discussed at the appointed time, and after being referred to a committee, which reported favorably, were adopted:

Whereas, few subjects of greater interest and importance could be presented to the consideration of the American Medical Association, now representing most of the States and Territories of the Union, than the attainment of a correct medical topography of each, with a history of its prevailing fevers and the most successful treatment of the same; therefore, be it

*Resolved*, That with this view and conviction, this Association now appoint a special committee from each State and Territory represented, of — numbers, whose duty it shall be to report upon its medical topography, epidemic fevers and the most successful treatment thereof, and that the same shall continue to hold their office for three years.

*Resolved*, That in the appointment of gentlemen of education and experience in the affairs of their own State, we have the best guarantee that the important objects we seek will be most satisfactorily accomplished, and the profession as well as the public interest will thereby be better served.

*Resolved*, That the committee heretofore appointed by this Association at its session in Charleston, for a similar object, be and the same are hereby discharged.

*Report of Publication Committee.*—Dr. Pliny Earle, of New York, Chairman of the Committee on Publication, made a report which states that 1000 copies of the 7th volume of the Report of the Association had been printed, at an expense of \$1806 42; of which number 781 copies had been furnished to members and sold, 35 copies furnished to medical journals, and 184 copies remained on hand.

*Report of Treasurer.*—The Treasurer, Dr. Isaac Wood, of New York, presented his report, showing that the sum of \$2101 04 was expended during the past year for the purpose of printing, binding, engravings, prize essays, etc.

The balance received from Dr. D. F. Condie, the former Treasurer, was \$293 99, while the assessments and sale of Transactions amounted to \$2722 31. There was also \$200 received from Drs. George B. Wood and Daniel Brainard, on account of the prize fund, thus leaving a balance on hand of \$1115 26.

Dr. Mauran, of Rhode Island, offered a series of resolutions returning the thanks of the Association to the Senators and Representatives who framed the Quarantine bill, now pending in Congress, to prevent sickness on ship board, including in the hope that their efforts may yet be crowned with success.

The resolution was passed.

The Secretary read a paper from Dr. W. H. Byford, of Evansville, Ind., on the pathology and treatment of scrofula, giving the nature of the disease, treatment, causes, effects, etc.

Dr. N. S. Davis, of Chicago, presented a report on the "Nutritive qualities of milk, and the influence produced thereon by pregnancy and menstruation in the human female, and by pregnancy in the cow; and also on the question whether there is not some mode by which the nutritive constituents of milk can be preserved in their purity and sweetness, and furnished to the inhabitants of cities in such quantities as to supersede the present defective and often unwholesome modes of supply."

The report having been read, was referred to the Committee on Publication, and the meeting adjourned.

### THIRD DAY.

The meeting was called to order punctually at 9 o'clock. The minutes having been read and approved —

The Chair said that there was no regular way of appointing the Committees on Prize Essays, Arrangements, etc.; it had been usual, however, for the convention to refer the appointment of these committees to the Committee of Arrangements.

Dr. Watson moved that the subject be referred to that Committee on this occasion.

The motion was agreed to.

A letter was read from Dr. Rayburn, resigning his office as Chairman of the Committee on Medical Topography, and asking the division of the territory composed of Wisconsin, Mississippi, Iowa and Illinois.

Dr. Watson moved the acceptance of the resignation, and the reference of the other portion of the communication to the Committee on Nominations, which was afterwards withdrawn.

Dr. Askew renewed the motion, and it was adopted.

Dr. Hays made a personal explanation, after which a letter was read from Dr. E. B. Haskins, of Clarksville, Tennessee, Chairman of the Committee on Microscopical Investigations of Malignant Tumors, asking to be excused from making a report, inasmuch as he had not the necessary apparatus for ascertaining the facts incident to the subject.

The request of the gentleman was granted.

Dr. Frank H. Hamilton, of Buffalo, made some additional remarks on his report delivered yesterday, and cited from the writings of the most celebrated surgeons both in this and other countries, to show that a majority of fractures of the clavicle, under any treatment yet known, result in deformity. He called upon the members of the Pennsylvania Hospital for statistics concerning their treatment of the fractured clavicle, and said he had known of one physician who had been mulcted in heavy damages because he did not use the same process as that adopted by this Hospital. He again urged upon them to bring forward the statistics, and thereby prevent us from being the grand insurance company of the world.

The Hon. Judge Lewis, Chief Justice of the State of Pennsylvania, being present, was unanimously invited to a seat on the platform.

Dr. J. G. Orton, of Binghamton, N. Y., offered resolutions requesting medical societies throughout the country to require their members to keep accurate records of their cases, and present annual digests of the same to this Association through the several State societies. Referred to the Committee of one from each State.

Dr. Condie, of Pennsylvania, stated that, although not quite finished, he was prepared to read his report on "the causes of tuberculous diseases." It, however, embodied a space of five hundred pages, and an abstract would require sixty pages. He could not expect the Association to publish it, but left it entirely with them to do as they pleased, for there would be no difficulty in its being brought out by medical publishers.

On motion, Dr. Condie was authorized to exercise his own discretion in the matter.

On re-assembling, Dr. Mussey, of Ohio, read an abstract of his report upon the "Influence of Alcohol in Health and in Disease." This report was considered as highly appropriate to the present state of excitement in regard to the temperance question; and the views of the venerable author seemed to us to coincide fully with those of the advocates for total abstinence. The report was ordered to the Publishing Committee.

Part of the report by Dr. Blatchford, of New York, on "Hydrophobia," was read by the Secretary and ordered to be published.

#### FOURTH DAY.

The minutes having been read and approved, resolutions of thanks to the Committee of Arrangements, the Mayor, the guardians of the public institutions, and the citizens and physicians of Philadelphia, for the courtesies extended to the members, were presented by Dr. Stewart, of New York, and Dr. Hayward, of Massachusetts, and unanimously adopted.

Dr. N. S. Davis, of Illinois, moved the following preamble and resolutions, which were referred to the Committee of Arrangements, with instructions to report on the same at the commencement of the next annual meeting:—

*Whereas*, The present mode of conducting the annual meetings of the Association affords but little opportunity for the discussion of strictly scientific questions

and papers, and whereas, this has been regarded as a serious defect in the operation of our organization, impairing its scientific character, therefore,

*Resolved*, That the daily sessions of the Association during each annual meeting be divided into two parts, the first to terminate at an hour not later than half past twelve o'clock P. M., of each day, and to be devoted, as heretofore, to the general business of the Association. The second part, consisting of all the time which it is deemed advisable to remain in session each day after half past twelve o'clock P. M., to take the character of a scientific section, and to be devoted exclusively to the discussion of questions relating to the science and art of medicine.

*Resolved*, That the Association in its capacity of a scientific section, having no power to act on any subject except of a scientific character, may continue in session, whenever thought desirable, a longer period than in its more general capacity.

A paper by Dr. Phelps, of New York, on the subject of religion in connection with medicine, which had been referred to a special committee, was reported upon, and ordered not to be published with the Transactions.

Dr. A. J. Semmes of Washington, D. C., offered the following, which was agreed to:—

*Resolved*, That a Committee of three delegates be appointed to report to this Association at its next annual meeting, what measures should be adopted to remedy the evils existing in the present methods of holding coroner's inquests, by incompetent persons, by which the lives and liberties of the innocent may be jeopardized, and the ends of justice frustrated.

Drs. Semmes, Tyler, and Condie, appointed.

Dr. J. L. Atlee, offered the following:—

*Resolved*, That to secure efficient teaching in medical schools where a prime object is to enforce practical precepts, a large degree of union and harmony must exist among the teachers, and confidence be reposed in them on the part of their pupils.

*Resolved*, That any such unnatural union as the mingling of an exclusive system, such as homeopathy, with scientific medicine in a school, setting aside all questions of its untruthfulness, cannot fail, by the destruction of union and confidence and the production of confusion and disorder unsettling and distracting the minds of the learners, to so far impair the usefulness of teaching as to render any school adopting such a policy unworthy the support of the profession.

The resolution was unanimously agreed to.

The action of the State Medical Society of Ohio, in refusing to acquiesce in some of the requirements of the Code of Ethics of the Association, was alluded to, and gave rise to an animated discussion: the course of the Society was generally condemned, and by none more than by members from Ohio.

Dr. Clendennin, of Ohio, offered the following:—

*Resolved*, That no State or local society shall be hereafter entitled to representation in this Association that has not adopted its Code of Ethics.

*Resolved*, That no State or local society that has intentionally violated or discarded any article or clause in the Code of Ethics, shall longer be entitled to representation in this body.

A motion to lay the above on the table was lost.

Dr. Miltenberger, of Baltimore, offered a resolution, which was amended by Dr. J. L. Atlee to read as follows:—

*Whereas*, It has been brought before the notice of the American Medical Association, that the State Medical Society of Ohio has violated at their last meeting, Section fourth, Article first, Chapter second of its Code of Ethics; therefore, be it

*Resolved*, That the Secretary of the Association be directed to inform the officers of that Society, that unless such action be rescinded, they cannot be hereafter represented in this Association.

These, together with the resolutions of Dr. Clendennin, were adopted.

Dr. Corson, of New York, presented and read a volunteer communication on the influence of lead on the heart. The paper was referred to Drs. Davis, Isaac Wood, and F. C. Stewart of New York.



Dr. Stillé, of Philadelphia, read a series of resolutions proposing to alter the mode of instruction in medical colleges, which were referred to a special committee to report on next year.

*Committee.*—Drs. A. Stillé, Prof. S. Jackson, John Bell, J. Watson, J. L. Cabell.

Dr. Thomas, of Baltimore, exhibited an apparatus for the inhalation of the impalpable powder of the nitrate of silver. Referred to a committee of three.

Numerous amendments to the constitution were proposed, and laid over under the rules for future consideration. Business having been completed, the Association adjourned at one and a half o'clock, *sine die*.

During its session the convention appointed the following committees :

*On Prize Essays.*—Drs. A. B. Palmer, Samuel Denton, A. R. Perry, Abram Sager, S. H. Douglass, C. La Ford, E. Andrews.

*Committee of Arrangements.*—Zina Pitcher, of Detroit, Chairman ; Moses Gunn, G. B. Russell, A. S. Leland, Moses Stewart, P. Klein, J. A. Brown, all of Michigan.

*Committee on Organization of State and County Societies.*—A. B. Palmer, Detroit ; N. B. Ives, Conn. ; E. B. Haskins, Tenn. ; C. Woodward, Ohio ; J. Crosby, N. H.

*Committee on Medical Education.*—W. H. Anderson, Ala. ; J. B. Flint, Ky. ; P. H. Cabell, Ala. ; G. Hayward, Mass. ; E. B. Smith, Mo.

*Committee on Medical Literature.*—P. C. Gaillard, S. C. ; N. P. Monroe, Maine ; J. Couper, Del. ; R. Hills, Ohio ; A. Coffin, S. C.

*Committee to procure Memorials of the Eminent and Worthy Dead.*—Dr. P. A. Jewett, Conn. ; T. F. Patton, Pa. ; C. J. Blackburne, Ky. ; W. M. Boling, Ala. ; Z. Pitcher, Michigan.

*Committee on Registration of Marriages, Births and Deaths.*—Dr. M. W. Wilson, Hartford, Conn., Chairman ; Drs. G. S. Palmer, Gardiner, Me. ; Silas Cumming, Fitz William, N. H. ; G. T. Elliott, Woodstock, Vermont ; Ed. Jarvis, Dorchester, Mass. ; Jos. Mauran, Providence, R. I. ; John H. Griseom, New York, N. Y. ; H. Carpenter, Lancaster, Pa. ; O. H. Taylor, Camden, N. J. ; Lewis P. Bush, Wilmington, Del. ; A. Snowden Pigott, Baltimore, Md. ; David H. Tucker, Richmond, Va. ; N. J. Pitman, Tarboro, N. C. ; Harvey Lindsly, Washington, D. C. ; John L. Dawson, Charleston, S. C. ; R. D. Arnold, Savannah, Ga. ; A. Lopez, Mobile, Ala. ; Jas. Jones, New Orleans, La. ; B. C. Foster, Nashville, Tenn. ; C. J. Blackburne, Covington, Ky. ; John Dawson, Columbus, O. ; Edward Murphy, New Harmony, Ind. ; A. D. Stebbins, Detroit, Mich. ; J. V. C. Blaney, Chicago, Ill. ; Geo. D. Wilbur, Mineral Point, Wis. ; Wm. McPheters, St. Louis, Mo. ; J. D. Elbert, Keosauqua, Iowa ; John H. Murphy, Falls of St. Anthony, Minnesota ; Mississippi and Arkansas blank.

*Committee on Medical Topography and Epidemics.*—J. C. Winston, of Bangor, Me. ; Edmund R. Peaslee, of Dartmouth College, N. H. ; Joseph Perkins, of Castleton, Vt. ; George C. Shattuck, of Boston, Mass. ; Joseph Mauran, of Providence, R. I. ; Chas. Hooker, of New Haven, Conn. ; Joseph M. Smith, of New York, N. Y. ; Jacob M. Gemmil, of Hollidaysburg, Penn. ; Lyndon A. Smith, of Newark, N. J. ; James M. Thompson, of Wilmington, Del. ; Peregrine Aiscoth, of Chestertown, Md. ; Thomas Miller, District of Columbia ; P. F. Peebles, of Petersburg, Va. ; O. F. Manson, North Carolina ; D. O. Cain, of Charleston, S. C. ; John F. Posey, of Savannah, Ga. ; S. W. Clanton, of Warsaw, Ala. ; T. O. Grafton, of Rodney, Miss. ; T. J. Fenner, of New Orleans, La. ; E. B. Hoskins, of Clarksville, Tenn. ; M. L. Sutton, of Georgetown, Ky. ; G. Mendenhall, of Cincinnati, O. ; Vierling Kersey, of Milton, Ia. ; J. H. Beach, of Cold Water, Mich. ; John Evans, of Chicago, Ill. ; S. B. Alleyne, of St. Louis, Mo. ; A. S. Castleman, of Delaware, Wis. ; E. A. Arnold, of Davenport, Iowa ; J. H. Murphy, Falls of St. Anthony, Minn. ; Thomas Dillard, of Philadelphia, U. S. N. ; Clement A. Finley, U. S. A.

*Special Committees to report upon named subjects.*—Lewis Steiner, of Washington, D. C., on strychnia—its chemical and toxicological properties.

Ashbury Evans, of Covington, Ky., on tracheotomy in epilepsy.

J. Taylor Bradford, of Augusta, Ky., on the treatment of cholera.

Charles Z. Chandler, of Rocheport, Mo., on malignant periodic fevers.



H. A. Johnson, of Chicago, Illinois, on the excretions as an index to the organic changes in the system.

Henry J. Bigelow, of Boston, Mass., on microscopical investigation of malignant tumors.

E. H. Davis, of New York, on the statistics of calculous diseases, and the operations thereof.

J. S. Carpenter, of Pa., on the treatment and curability of redicible hernia.

N. Fuller, of Maine, on the best treatment of cholera infantum.

William B. Page, of Philadelphia, on injuries of the joints.

T. Wilson Jewell, of Philadelphia, on the statistics of mortality in the United States.

J. Knight, of New Haven, Conn., on endemic fevers.

P. H. Cabell, of Ala., on the native substitutes for cinchona, indigenous to the Southern States.

James M. Newman, of Buffalo, New York, on the sanitary police of cities.

L. M. Noble, of Le Roy, Ill., on puerperal fever and its communicability.

W. J. Freer, of Chicago, Ill., on the progress of general and descriptive anatomy.

J. W. Corson, of New York, on the causes of the impulses of the heart, and the agencies which influence it in health and disease.

D. M. Reese, of New York, on the causes of infant mortality in large cities, the source of its increase, and the means for its diminution.

Mark Stephenson, of New York, on the treatment best adapted to each variety of cataract, with the method of operation, place of election, time, age, etc.

J. B. Coleman, of New Jersey, on the effect of mercury on the living tissues.

F. G. Richardson, of Louisville Ky., on the diversity of venereal poison.

J. B. Flint, of Louisville, Ky., on the best mode of rendering the medical patronage of the national government tributary to the honor and improvement of the profession.

M. M. Latta, of Goshen, Indiana, on whether there are any means by which the growth of the fetus in utero may be controlled without injury to the mother or child.

Thos. Miller, of Washington, on toxicology.

E. K. Peaslee, of Hanover, N. H., on inflammation, its pathology, and its relations to the reparative process.

D. D. Thompson, of Louisville, Ky., on the remedial effects of chloroform.

Wm. Clendennin, of Cincinnati, Ohio, on epidemic erysipelas.

C. G. Comegys, of Cincinnati, Ohio, on the state of the urine in tubercular disease.

During their stay in Philadelphia, the members of the Association were most hospitably entertained by their colleagues of that city. Evening parties were given to them by Drs. Wood, Stillé, Paul, S. Jackson, Pancoast, Hartshorne, Bache, Hodge, and Mr. Lee. All the public, and many private institutions were thrown open for their inspection, and every available moment was agreeably occupied. All seemed delighted with their visit, and the utmost harmony and good feeling everywhere prevailed.

CHOLERA—*Castor Oil in*—The *coup de grace* has been given to the castor oil treatment of cholera, by a Committee of the London Board of Health, who thus close their report:

"From the above abstract, the details of which have been carefully investigated by the Committee, it appears that in eighty-nine cases of cholera treated by different practitioners with castor oil, on the plan recommended by Dr. Johnson, sixty-eight were fatal, recovery having occurred in only fifteen cases, whilst six are still under treatment."

The same experiment was made with this "new remedy" in the Dreadnaught Hospital ship, where out of nineteen cases twelve died, whilst in the Glasgow Hospital, after losing seven out of nine patients, the treatment was abandoned.—*Virginia Med. & Surg. Journal*.

ON THE TREATMENT OF SCIATICA BY CROTON OIL. — Henry Hancock, Esq., Surgeon to the Charing-Cross Hospital, advances the idea that the majority of cases of sciatica are dependent on "mechanical irritation of the pelvic nerves, from loaded intestines, or tumors within the pelvis, and not upon rheumatism or inflammation of the nerve, as was commonly supposed." He gives a pill composed of castor oil, blue mass, extract of henbane, and compound extract of colocynth, and adduces cases in illustration of the efficacy of the prescription. — *London Lancet*.

MEDICAL TEACHING IN TURKEY. — M. Scoultetén, chief physician to the hospital in Constantinople, lately visited the Turkish Military School of Medicine, accompanied by several surgeons of the French army. This vast establishment contains about five hundred students, who are admitted at the age of six years, thoroughly instructed in the rudimentary branches of a good education, as well as in the French, Latin and Greek languages. The medical course lasts five years, at the end of which time the students pass their examination and receive their diplomas. The Sultan being always present on the occasion of this important ceremony. The students are educated, fed, lodged and clothed at the expense of the State, and receive, moreover, a monthly stipend, varying according to age, the highest pay being about eight dollars. At the termination of their studies they enter the army as officers.

What is somewhat surprising, is the fact that the lectures are given in the French language.

AMMONIA IN POISONING BY OPIUM. — M. Costa, in the *Réview de Thérapeutique Médico Chirurgicale*, advises the administration of ammonia in cases of poisoning of this kind. He mentions the case of a man who had taken — of laudanum which had remained on the stomach six hours without causing death. This, he says is attributable to the fact that the opium brought about paralysis of the stomach, which prevented in part the absorption of the poison. A few drops of ammonia being administered, violent vomiting ensued, and the functions of animal life were partly restored. As soon as the vomiting ceased the patient relapsed into insensibility. On being again given, the vomiting recurred. This was kept up until the patient had got rid of his dose.

M. Costa believes that the ammonia acts in these cases as an emetic and excitant of the nervous system, and concludes that it is the first remedy to which recourse should be had in such cases.

A SUBSTITUTE FOR CINCHONA. — M. Amic, physician at Martinique, has lately been making use of the bark of a certain tree which flourishes in that country, in the treatment of fevers; he reports its febrifuge qualities equal to those of cinchona, and recommends it as a substitute. The name of the tree is not given. The attention of the Governor had been directed to the fact, and he has appointed two capable persons, M. Chapins and M. Girardias, to make a study of the tree and report upon its medicinal properties.

Our Journal has now such a list of subscribers, as to make it a valuable medium of advertising to Druggists, Surgical Instrument makers, Bandagists, and others, whose occupations are either remotely or directly connected with the profession. We therefore place before them the terms of the New Orleans Medical News and Hospital Gazette, and solicit their patronage.

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NEW ORLEANS

MEDICAL NEWS AND HOSPITAL GAZETTE.

VOL. II.

AUGUST 1, 1855.

NO. VI.

*Speculations on the Cause of Yellow Fever.*

*By JOHN HARRISON, M. D.,*

*Professor of Physiology and Pathology in the Medical College of Louisiana.*

[CONCLUDED.]

Gaspard, following up his experiments, showed that putrid liquids injected into the cavity of the pleura, etc., produced the same effects as injections into the veins. Upon Magendie's experiments, he makes the following observations :

"Magendie, indeed, has not discovered this absorption to be very injurious to the pulmonary, gastric or intestinal mucous surfaces; but in my opinion, this would have become so, had the experiments been continued for a much longer time. This distinguished Physiologist should have been less struck perhaps by this result, than any other, since he had observed on a former occasion, that the most energetic vegetable poisons are absorbed more feebly and cause death more slowly on the application of them to mucous surfaces than on serous, or even by being introduced into various organs. Such is the case with the inoculation of the venom of the viper as compared with its ingestion, likewise with the poison of Indian arrows, so pernicious in wounds, but slightly so in the stomach. It becomes us in this instance to admire the protecting wisdom of nature, for if putrid or any poisonous substances whatever, could have been readily absorbed by the respiratory or digestive passages, there is no doubt, that the species of carnivorous animals, as an example, that so often live on rotten flesh for want of fresh, would no longer be in existence; by another wise precaution, they have received very short intestines, compared with those of herbivorous an-

imals, in order, perhaps, that their putrid excrements should be retained for a very little while, and might not have time to become fatal. Moreover, there is no doubt, that but for this difficult absorption of putrid bodies, the human race itself would have been decimated oftener than it is, and that, for example, the lovers of tainted meat and strong game, would have become its victims on every indulgence, as well as those, who breathe the emanations of putrid bodies. Nevertheless, in spite of this admirable precaution, men and domestic animals but too generally succumb to these terrible maladies, especially epidemics, which bear the greatest resemblance to those produced artificially in animals by the injection of putrid liquids into their veins, the causes and symptoms of which are traceable entirely to putrefaction. In all these diseases, the blood plays the chief part and becomes the essential seat of the evil; it is manifestly altered, very black as well as viscid, to a great extent robbed of its plasticity and its fibrine, and very different from what it is in febrile and inflammatory affections. Under these diseases, it exudes from the economy by a species of vascular transudation, after the manner of what is called passive hemorrhages, which continue or happen even after death, under the form of petechiæ or ecchymoses, etc. In all these maladies, in a word, there are general and local symptoms of putrescence, such as fetid breath, urine, sweat, alvine dejections, etc.; such also as meteorism, emphysema, the disengagement of gas in the intestines, in the cellular tissue, and even in the blood;—such, in short, as anthrax, partial gangrene, softening of the flesh and the very rapid decay of the dead body. At least, this is what numerous and credible observers have well established in scurvy, plague, the yellow fever of Siam and America, the adynamic or putrid fever, fitly so called, various forms of typhus and pernicious fevers, in gangrene, gangrenous sore throat, dysentery, in gangrenous ergotism, carbuncle, in the epizotic form of carbuncula fever, and finally in malignant pustule. We might add to this list of putrid disorders, that which hunger causes in its last degree; that which results from the abuse of mercury;—in short, that which the venom of many snakes produces.”

The last extract, which we shall make upon the subject, is from Christison.\* It would be easy to extend these extracts, but perhaps more than enough have already been given.

“The tendency of putrefaction to impart deleterious qualities to animal matters, originally wholesome, has been long known, and is quiet unequivocal

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\* Christison on Poisons, p. 490: Amer. Edit.



cal. To those, who are not accustomed to the use of tainted meat, the mere commencement of decay is sufficient to render meat insupportable and noxious. Game, only decayed enough to please the palate of the epicure, has caused severe cholera in persons not accustomed to eat it in that state. The power of habit, however, in reconciling the stomach to the digestion of decayed meat, is inconceivable. Some epicures in civilized countries prefer a slight taint even in their beef and mutton : and there are tribes of savages still further advanced in the cultivation of this department of gastronomy, who eat with impunity rancid oil, putrid blubber, and stinking offal. How far putrefaction may be allowed to advance without overpowering the preservative tendency of habit, it is not easy to tell. But with the present habits of this and other civilized nations, the limit appears very confined.

Putrid animal matter, when injected into the veins of healthy animals, proves quickly fatal ; and from the experiments of Gaspard and Magendie, together with the more recent researches of MM. Leuret and Hamot,\* the disease induced, seems to resemble closely the typhoid fever of man.

Similar effects were observed by Magendie, when dogs were confined over vessels, in which animal matter was decaying, so that they were obliged always to breathe the exhalations. These discoveries throw some light on the question regarding the tendency of putrid effluvia, to engender fever in man ; and notwithstanding many well ascertained facts of an opposite import, they show, that probably in peculiar circumstances, decaying animal matter may excite epidemic fevers. A detailed investigation of this important topic would be misplaced here, as it belongs more to medical police than to medical jurisprudence ; but the two works quoted below are referred to for examples, in my opinion, of the unequivocal origin of continued fever in the cause now alluded to ;† and other instances of the like kind will be found in the Report of the Parliamentary Commission on the health of towns.

Another affection sometimes brought on by putrid exhalations, is violent diarrhœa or dysentery, of which a remarkable instance lately occurred in the person of a well-known French physician, M. Ollivier. While visiting a cellar where old bones were stored, he was seized with giddiness, nausea, tendency to vomit, and general uneasiness ; and subsequently he suffered from violent colic with profuse diarrhœa, which put on the dysen-

\* *Journal des Progres des Sciences Medicales*, 1827, v. i. 181.

† De divers accidens graves, occasions par les miasmes d'animaux en putrefaction. *Mem. de la Soc. de Med.*, v. i, 97.—*London Med. Chirurg. Review*, v. i. 202.

teric character and lasted for three days.\* Chevallier, in noticing this accident, mentions his having been affected somewhat in the same way when exposed to the emanations of dead bodies; and it is a familiar fact, that medical men, who engage in anatomical researches after long disuse, are apt to suffer at first from smart diarrhoea. The same remark must be applied here as at the close of the observations in the last section. Without peculiar concurring circumstances, no bad effect results. This will follow from many facts illustrative of the innocuous nature of various trades, where the workmen are perpetually exposed to the most noisome putrid effluvia. But no facts of the kind are so remarkable as those collected in regard to the establishment at Montfaugon by Parent-Duchatelet, who makes it appear, that this most abominable concentration of the worst of all possible nuisances is not merely not injurious to the health of the men and animals employed in and around it, but actually even preserves them from epidemic or epizootic diseases.†

The effects of putrid animal matter, when applied to wounds, have been investigated experimentally by Professor Orfila, who found, that putrid blood, bile, or brain, caused death in this way within twenty-four hours,—producing extensive local inflammation of the diffuse kind, and great constitutional fever. In man also, several instances of diffuse cellular inflammation have been observed as the consequence of pricks received during the dissection of putrid bodies. The disease, as formerly observed, certainly arises in general from pricks received in dissecting recent bodies. At the same time, a few cases have been traced quite unequivocally to inoculation with putrid matter,‡ and if any doubts existed on this point, the experiments of Orfila would remove them.

Mr. Lassaigue has examined chemically the putrid matter formed by keeping flesh long in close vessels, and has found it to consist of carbonate of ammonia, much caseate of ammonia, and a stinking volatile oil,—the last of which is probably the poisonous ingredient.

The theory then, of the etiology of yellow fever, may be thus stated: From the accumulation of filth in large cities (chiefly night-soil and the animal matter of urine,) putrefaction must necessarily take place, and from this putrefaction, *under certain meteorological conditions*, there is generated a poison, which, either in the form of a volatile oil, or other organic matter, held in solution by ammonia, floats in the atmosphere; is inhaled

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\* Annales d'Hyg. Publique et de Med. Legale, v. ii. 216.

† Annales d'Hyg. Publique et de Med. Legale, v. iii. and ix.

‡ Dr. Duncan, Edin. Med. Chirurg. Trans., i. 502 and 520.

during the respiratory movements; is taken into the circulation and poisons the system. It produces specific effects, as much so, as the matter of small-pox or scarlatina.

The formation of this poison begins under certain meteorological conditions, which are utterly unknown to us, continues while they last, and ceases with them. As we have said before, the poison is not a gas, but a volatile substance, constituted of organic matter, as much so constituted, as the matter of small pox or hydrophobia.

To these views certain objections have been offered, which we shall now reply to.

1st. It is objected, that the closest chemical analysis of the atmosphere can detect no such substances. The chemists themselves must answer this objection, and for this purpose I shall select the authority of Sir Humphrey Davy, of Mulder and of Raspail.

The opinions of Sir H. Davy are uttered from the lips of one, whom he chooses to call 'a stranger,' but are evidently his own.

"I said, 'I believe several scientific persons, Brocchi amongst others, have doubted the existence of any specific matter in the atmosphere, producing intermittent fevers, in marshy countries and hot climates; and have been more disposed to attribute the disease to physical causes, dependent upon the great differences of temperature between day and night, and to the refrigerating effects of the dense fogs, common in such situations, in the evening and morning; and, on this hypothesis, they have recommended warm woollen clothing and fires at night, as the best preventives against these destructive diseases, so fatal to the peasants who remain in the summer and autumn in the neighborhood of the maremme of Rome, Tuscany, or Naples.'"

"The stranger said, 'I am acquainted with the opinions of the gentlemen, and they undoubtedly have weight; *but, that a specific matter of contagion has not been detected by chemical means, in the atmosphere of marshes, does not prove its non-existence.* We know so little of those agents that affect the human constitution, that it is of no use to reason on this subject. There can be no doubt that the line of malaria, above the Pontine marshes is marked by a dense fog morning and evening, and most of the old Roman towns were placed upon eminences out of the reach of this fog. I have myself experienced a peculiar effect upon the organs of smell in the neighborhood of marshes in the evening after a very hot day; and the instances in which people have been seized with intermittents by a single exposure, in a place infested by malaria in the season of fevers, gives, I think, a strong support to something like a poisonous material existing in the at-

mosphere in such spots; but I merely offer doubts. I hope the progress of physiology and of chemistry will at no very distant time solve this important problem.”\*

The italics are my own. Instead of ‘a specific matter of *contagion*,’ it is obvious that the words should be ‘a specific poison.’

I shall now cite Mulder. “Finally, Boussaingault, Verver, and others, have shown the presence in the atmosphere of the substances, which contain hydrogen and carbon. Boussaingault and Verver passed atmospheric air, *freed from carbonic acid*, over red-hot copper, and obtained small quantities of water and carbonic acid. By means of the oxygen of the air, the hydrogen and the carbon—no matter in what state they existed in it—would be changed into water and carbonic acid, while passing together over red-hot copper.

“We cannot determine in what state this hydrogen and carbon are contained in the atmosphere; they may be so, in the form of hydrogen gas, carburetted hydrogen, and carbonic oxide, or possibly in that of volatile organic substances. As to this point, nothing has yet been ascertained, nor can it be determined by such experiments as these. It is certain, however, that before the constituents of organized bodies are reduced to their most intimate combinations, they can assume a great many intermediate states—supplying the atmosphere with either solid, liquid, or gaseous products. Thus, in every kind of putrefaction, peculiar volatile substances are diffused through the air, which may contain the four organic elements combined in various ways. Further, in a great many diseases—such, for example, as cause eruptions on the skin—volatile compounds escape from the patients, and are diffused through the atmosphere.† Many substances, also, which, both at ordinary and at more elevated temperatures, are regarded as fixed and not volatile, are constantly emitting particles in a state of vapor, and diffusing them through the atmosphere—such for instance are potash, soda, and even iron, when smelted in blast furnaces. Thus, the atmosphere must contain not only substances consisting of carbon, hydrogen, nitrogen, and oxygen, but a great many others besides, which it would really be of

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\* Consolations in Travel, etc.

† See on this subject the elaborate treatise of Dr. Van Guens, *Natuur-en Geneeskundige Beschouwingen van moerrassen en moerasziekten*. Amsterdam, 1839. (Physical and Medical considerations on Marshes and Marsh Diseases. Amsterdam, 1839.) Mulder's *Verhandeling over de Wateren en de Lucht van Amsterdam*, (Treatise on the waters and the air of Amsterdam,) p. 163, may be also consulted on this subject.



importance to investigate, but on which it is unnecessary for our present purpose to dwell.”\*

Raspail's language is still stronger. “The eudiometric analysis of the atmospheric air,” says he, “will show a certain invariableness and a certain uniformity in its principal constituents in whatever place, and at whatever elevation, observation has carried this instrument of analysis. It will result from it that the air is a constant mixture or a combination in weight of 21 parts of oxygen, and 79 parts of nitrogen, together with a variable quantity of watery vapor, and an exceedingly small quantity of carbonic acid—a quantity more variably than the first.

Without doubt, it is permitted us to consider this analytic composition as representing the normal condition of the atmosphere,—that which suffices for, and is best suited to organic development. But it is repugnant to logic and to observation, to admit it as the constant and invariable constitution of a medium, which at every instant, is the receptacle and absorbent of so many, and such various gaseous emissions;—experiment, in this respect, with all its graphic apparel of exactness and precision, does wrong to analogy.

How, in fact, suppose that the air of a theatre during the representation can be as simple in its composition as that in the glades of the forest? Is it not contradictory in terms to admit that the air which is breathed on the borders of swamps at the season when the miasms cause fevers, is composed of only the four elements which we breathe every where else for the maintainance of our general health? How vanish away then these ammoniacal, phosphorescent, sulphurous, hydrocyanic emanations, etc., which are discharged into the air from thousands of gaping apertures, such as our privies, our manufactories, our chimnies, our sewers?—from all that ferments and is decomposed; from every one who breathes and gives back to the atmosphere a disoxygenated air impregnated with all the vapors which the respiratory surfaces exhale? Why then has not analysis discovered these emanations? Because, at first, she has never thought about them. She has begun to tread this path since we have pointed it out, and to be conscious that the former processes possessed but an apparent and conventional precision. In fact, to estimate the respective quantities of oxygen and nitrogen, recourse is had either to detonation by electricity or to the action of phosphorus. In the first case, to the air employed, there is added a

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\* The Chemistry of Vegetable and Animal Physiology, by Dr. J. G. Mulder, etc. Amer. edit., p. 101.

quantity of hydrogen greater by 42 volumes ; the eudiometer is exploded, and the quantity of oxygen is computed by the quantity of hydrogen transformed into water ; the quantity of nitrogen being estimated by the difference. In the second case, a stick of phosphorous is introduced into the instrument for the purpose of absorbing the oxygen and transforming it into phosphoric acid. The gaseous portion not absorbed, represents a mixture of nitrogen and carbonic acid. To absorb this last, a solution of fixed alkali is used, and the analysis goes no further. The volume remaining, separated from its oxygen by the phosphorous or by the explosion, and from its carbonic acid by the potash, can be nothing else than the nitrogen.

Now let us suppose that the volume of atmospheric air subjected to experiment had contained other gaseous elements in a state of combination or mixture ; let us examine if, by such processes as the preceding, the analysis could detect them. Some examples will suffice to show us the value of such a supposition. Admit, that the air contains a certain quantity of free ammonia ; the phosphorous will convert it into fixed phosphate of ammonia, absorbing oxygen at the same time. If the ammonia exist, as an alkaline salt with excess of base, the phosphorous, now become phosphoric acid, will fix this salt, by saturating it and transforming it into a double salt of ammoniacal base. But in both cases, this quantity of ammoniacal gas will go, unknown to the experimenter, to the account of the oxygen. Admit in the air the existence of some acid emanation, of any nature whatsoever ; this acid, in the proof with potash, will pass to the account of carbonic acid. Finally, the gases which neither phosphorous nor potash can absorb—sulphuretted and carburetted hydrogen, carbonic oxide, etc., neutral volatile salts, etc.,—all these will go to the account of the nitrogen, the residue of the analysis, which the analysis measures but does not seek to absorb or decompose.

Consequently the atmospheric air is not, at every moment, as pure as the eudiometric analysis would seem to indicate. Doubtless the existence of these emanations into the air cannot be neither permanent nor invariable ; and it must be admitted, that the electric power of the solar ray,—that the lightning which ploughs through the immense eudiometer of the atmosphere—combines, decomposes in a thousand different ways these elements, already so diverse. Why should it be otherwise in the expanse of the atmosphere, than in the vessels of our laboratories ? ”\*

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\* *Histoire Naturelle de la Sante et de la Maladie* ; tom. i. p. 40.

So much for this objection, which is so frequently repeated and so confidently relied on.

Indeed, from certain facts recently brought to light, it would appear, that we have good reason to hope, that the special poison of yellow fever may itself be detected and chemically examined. We are told by Liebig, that "all the observations hitherto made upon gaseous contagious matters prove, that they also are substances in a state of decomposition. When vessels filled with ice are placed in air impregnated with gaseous contagious matter, their outer surfaces become covered with water, containing a certain quantity of this matter in solution. This water soon becomes turbid, and in common language putrefies, or, to describe the change more correctly, the state of decomposition of the dissolved contagious matter is completed in the water."\*

I have already spoken of the probability of this poison being held in solution by ammonia. The following quotation bears upon that point:

"The odor of gaseous contagious matters is owing to the same cause; (i. e. *eremacausis*, or oxidation at common temperatures,) but it is also generally accompanied by ammonia, which may be considered in many cases as the means through which the contagious matter receives a gaseous form, just as it is the means of causing the smell of innumerable substances of little volatility, and of many which have no odor.

Ammonia is very generally produced in cases of disease; it is always emitted in those in which contagion is generated, and is an invariable product of the decomposition of animal matter. The presence of ammonia in the air of chambers, in which diseased patients lie, particularly of those afflicted with contagious disease, may be readily detected; for the moisture condensed by ice in the manner just described, produces a white precipitate in a solution of corrosive sublimate, just as a solution of ammonia does. The ammoniacal salts also, which are obtained by the evaporation of rain water after an acid has been added, when treated with lime, so as to set free their ammonia, emit an odor most closely resembling that of corpses, or the peculiar smell of dunghills."†

In a paper published by Dr. Hort in this Journal, (Jan. 1846) we also meet with some very interesting remarks, and some experiments performed by Prof. Riddell. As they are relevant, I quote them.

"It has been established by the experiments of Moscati and Boussingault, that organic matter exists in extremely small quantities in the nox-

\* *Agricultural Chemistry*. Amer. Edit., p. 407.

† *Op. cit.*

ious air that hovers over marshes. Moscati, many years ago, suspended in the air, over the rice grounds of Tuscany, a globular glass filled with ice. An abundant deposition of dew took place upon its surface, which, when collected, appeared at first to be pure limpid water. There was soon, however, an appearance of little flakes, possessed of properties peculiar to animalized matters, and finally, at the end of some days, the liquid putrefied completely. (*Riddell*.)

"These facts are testimony directly to the point, and they are not yet exhausted. In a memoir read before the French Academy of Sciences, in 1834, M. Boussingault reports some striking experiments tried by him at Cartago, in South America. In the middle of a swampy meadow, in every instance, carbonaceous matter was detected in the dew, by the addition of sulphuric acid. He remarks: "The results obtained prove very clearly that in marshy places, during the precipitation of dew, there is an organic matter." (*Riddell*.)

"The following experiments made by Professor Riddell, "with a view of detecting the aerial miasm of small pox," are so much in point, and so interesting, that I should do injustice to the subject were I to omit to quote them. The apparatus is thus described: A perfectly clean ounce phial was half filled with distilled water; a small glass tube, with a capillary orifice, was made to terminate near the bottom, the upper and much larger portion of the tube bending horizontally to receive the silver nozzle of a delicate pair of bellows; several turns of gauze were passed around the mouth of the phial, embracing the tube, and was carried to the city\*

\* Cincinnati.

pest house, and under the superintendence of Dr. Herron, it was placed on a table two or three feet from a small-pox patient, just in that stage of the disease when the circumambient air was supposed to be most contagious. The bellows were blown by the nurses pretty constantly for twelve hours, thus presenting a great amount of noxious air to the distilled water. The apparatus was left undisturbed until it came into my possession, three days after, when I made the following experiments:

"1. One-fourth of a drachm of the water contained in the phial, evaporated very slowly in a watch glass, over an alcohol lamp, left concentric circles of a whitish substance. Upon bringing this residue under the object glass of a good microscope, I discovered that it consisted mostly of long crystals, which shot from each other at right angles. The outer margin of each concentric band was less distinctly crystalline, and evidently contained some other substance.

"2. A minute drop of sulphuric acid, carefully distilled and collected on a glass rod, so as not to leave the slightest trace when evaporated from clean glass, was placed upon some of the residue. [Experiment No. 1.] The application of heat rendered the acid black, and upon complete evaporation a dark stain was left, thus showing the presence of organic matter.

"3. Upon adding a drop of pure sulphuric acid, to near an eighth of a drachm of the water, and expelling the water by a careful heat, the acid became black. This experiment, as well as the one which follows, was performed upon a piece of Florence flask, rinsed in clean water, and then heated to redness over an alcohol lamp, in order to remove every trace of organic matter.



"A drop of the water hastily evaporated, left a whitish residue, not crystalline to appearance, but consisting of extremely minute grains. Upon the application of a high heat short of redness, it became dark colored, indicating the presence of organic matter by the charcoal liberated. A still higher heat, in contact with air, removed the dark color, and left a mere trace of white adherent powder."

Now, though we do not at all believe yellow fever to be contagious, still, it is plain, that if its cause be a poison floating in the atmosphere, we ought not to despair, that it may be detected, when chemical analysis is further advanced. Add to this, the fact that Lassaigne detected, in his analysis of putrid meat-water, a stinking volatile oil, which he presumed to be the poisonous agent, which produced the singular effects developed in the experiments of Gaspard and Magendie; and the probability of detection is still further enhanced.

2. It may again be objected, 'that yellow fever in men is not caused by injections into the veins, and that, therefore, the experiments of Gaspard and Magendie are inconclusive. Certain similar effects are doubtless produced in these experiments, but death takes place too soon,—the symptoms are too rapid and violent, to constitute such affections entitled to the name of yellow fever—at all events, there is but a crude similitude;—the animal is poisoned in a peculiar manner, but we can predicate nothing more.

In reply, we may say, that, admitting the facts concerning the rapid and fatal symptoms, they so closely resemble those of yellow fever, and the post-mortem lesions of the two are so much alike, that we must infer similar causes have been in operation. The rapidity with which death ensues is doubtless owing, partly to the manner in which the poison is introduced, partly to the concentration of the poison. That such is the case, we learn from Magendie, who states, that when the poisonous liquid was placed in the stomach or alimentary canal, or injected into the trachea, there were scarcely any effects produced, and also, that when the poisonous matter was filtered, its effects were much less violent and the animals survived much longer.

3. It is again objected, 'that, even after Magendie's own showing, when the animal merely breathed the effluvia, though it died, there were no symptoms of yellow fever,—they were rather those of typhoid fever. Therefore, we cannot with propriety contend, that mere exhalations from putrefied matter are capable of producing yellow fever. The experiment, indeed, which Magendie says he has often repeated, seems to prove the contrary.

The conclusion is hasty and much too general in its tenor. It is not contended, that *all exhalations* from putrid organic matter are capable of producing yellow fever, but that it is caused by emanations of a peculiar kind, produced under peculiar circumstances.

Although putrid matters injected into the veins cause even more violent effects than we usually witness in yellow fever (these effects bearing all the strongest characteristics of that disease); though mere typhoid is generated by inhalation, still it does not follow, that, under other conditions of putrefaction, a poison may not be generated sufficiently strong to produce yellow fever;—at least in man. It comes to this at last:—that a poison is generated by the decomposition of nitrogenized organic matter, which, if injected into the veins of animals, causes speedy death with peculiar symptoms and post-mortem lesions, strongly resembling those of yellow fever; that if this poison be injected in a less concentrated state, sickness ensues with similar symptoms—ending in death, but at a longer interval. If the poison be merely inhaled, death ensues, but the symptoms are modified—instead of those of yellow fever we have typhoid. But of the existence of the same identical poison operating in all these cases, there can be no doubt.

4. Another objection is, “that our domestic animals are not subject to yellow fever.”

It is a common opinion in New Orleans, that animals recently imported, are, generally speaking, indisposed during an epidemic: but be this so or not, it proves nothing. For, it is a well known fact, that all races of animals and even individuals of the same race are not equally susceptible of morbid influences. A striking difference in this respect may be witnessed between the negro and the white, during an epidemic of yellow fever; and even among individuals of the white race. Some have the disease in its most malignant form—some have it very mildly—some, not at all. There are peculiarities of constitution, etc., which make the difference. It is probable, that if a human being had been placed in the situation of Magendie’s dogs over the grating, they would have perished much sooner and with more violent symptoms.

5. The next objection has been much insisted on. Our attention is called to the fact that butchers, *vidangeurs*, knackers, etc., who live in the midst of the most disgusting filth, are not only not subject to epidemic diseases more than other people, but, in fact, appear to be more exempt from them. “In the year 1828, a committee was appointed in Paris, to

inquire into the circumstances connected with the knacker's operations. Every one, examined by the committee, agreed, that they were offensive and disgusting, but no one that they were unwholesome. It was even inferred that they were conducive to health. All the men, women, and children, concerned in the works, had unvarying health, were remarkably well in appearance, and strong in body. The workmen commonly attained old age, and were generally free from the usual infirmities that accompany it. Sixty, seventy, and even eighty were common ages. Persons living close to the places, or going thither daily, shared these advantages with the workmen. During the time that an epidemic fever was in full force at two neighboring places, not one of the workmen in the establishment of Montfauçon was affected by it. Nor did it seem that this freedom from disease applied altogether to the men that were habituated to the works; for when from press of business, new workmen were taken into the establishment they did not suffer in health from the exhalations."\*

Of that delightful place, Montfauçon, Christison gives the following description. "The Voirie et Chantier d'Ecarissage of Montfauçon, which has existed close to the walls of Paris, for several centuries, is an enclosure of many acres, where the contents of the necessities of the city are collected in enormous pits, and where horses, dogs, and cats are flayed to the amount of forty or fifty thousand annually. The fat is melted for blow-pipe lamps; the bones are in a great measure burnt on the premises for fuel; the intestines are made into coarse gut for machinery; the flesh, blood, and garbage are heaped to putrefy for manure; and in summer a bed of compost is spread to breed maggots for feeding poultry. There is no drain. Description cannot convey an idea of the stench. The committee of the Board of Health, appointed to make inquiries into the best mode of abating the nuisance, in vain attempted to penetrate into the place. Yet the workmen and their families are stout, healthy, and long lived."†

We must admit the health of the workmen, as the fact is established on good authority, but we cannot admit there is any thing preventive of disease in the abominable emanations of such a place. It is plain that the epidemic fever spoken of as raging in two neighboring places, could not have arisen from the decomposition of animal matter. There are various causes of epidemic diseases, of which organic putrefaction is one, but not the only one. Situations, as pure as possible, might have been equally exempt from these epidemics.

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\* Dunglison's *Elements of Hygiene*, p. 108

† *Op. cit.* p. 490, Amer. Edit.

But admitting the healthiness of this and similar places; what does it prove? At the best, it is but a negative fact. Because disease is not generated at Montfaucon, does that prove that animal putrefaction in other latitudes, or under different circumstances, cannot generate it? Not at all. On the other hand, we have abundance of *positive* proof that many and exceedingly malignant disorders have been traced directly to such causes. Take the following instance: "According to Percy, a Dr. Chambon was required by the Dean of the Faculté de Médecine of Paris, to demonstrate the liver and its appendages before the "Faculty," on applying for his license. The decomposition of the subject, given him for demonstration, was so far advanced, that Chambon drew the attention of the Dean to it, but he was required to go on. One of the four candidates, Corion, struck by the putrid emanations which escaped from the body as soon as it was opened, fainted, was carried home, and died in seventy hours: another—the celebrated Fourcroy—was attacked with a burning exanthematous eruption; and two others, Laguerenne and Dufresnoy, remained a long time feeble, and the latter never completely recovered. 'As for Chambon,' says M. Londe, 'indignant at the obstinacy of the Dean, he remained firm in his place; finished his lecture in the midst of the commissioners, who inundated their handkerchiefs with essences, and doubtless owed his safety to his cerebral excitement, which, during the night after a slight febrile attack, gave occasion to a profuse cutaneous exhalation.'"\*

One such fact must overthrow a thousand which possess a mere negative character.

It does not follow by any means that because the nostrils are offended by disgusting odors that the putrescent mass must also emit substances deleterious to human health. But the great fallacy of this objection consists in supposing the putrefactive process and its results to be invariably the same. Never was there a greater mistake.

All organic substances capable of undergoing the putrefactive process are highly complex in their chemical constitution; of all bodies the most so. The process indeed belongs to a class of chemical actions altogether different in form and manifestation from ordinary decomposition. There occurs a transformation among the organic molecules into simpler compounds, sometimes with the accession of oxygen and water; sometimes without. But what these compounds shall be, is dependent upon a thousand circumstances. The slightest variation in the quantity of heat, of

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\* Dunglinson, op. cit., p. 110.



light, of moisture, of more or less of oxygen, even the character of the bodies in contact with the substances undergoing the process, exercise a wonderful influence, and give rise to entirely different products. This, perhaps, will be made more apparent by the language of Liebig:

"The juices of carrots, beet-roots, onions, etc., are rich in saccharine matter; when fermented at common temperatures they yield the same products as the juice of the grape, namely, carbonic acid and an alcoholic fluid, and the nitrogenous constituent of the juice is deposited in the form of a sediment or dregs. At a higher temperature, from 100 to 120 degrees for instance, the manner of the whole transformation is changed. There is less evolution of gas, and no alcohol is formed. When the process of fermentation is completed, and we examine the fluid, we find none of the sugar which was originally present; its elements have become transformed into a large proportion of lactic acid; and besides this there is a substance perfectly analogous to gum, and another substance, which, moreover, is the most remarkable of these products, of a crystalline appearance, identical in properties and constitution with the sweet constituent of manna.

"*Alcohol and Carbonic acid* are the products of the transposition of sugar atoms at the common temperature. At a higher temperature the results of the transformations effected by fermentation are, carbonic acid, mannite, lactic acid, and gum.

"We have every reason to believe that at high temperatures the manner of transformation in the elements of the substance exciting fermentation (the ferment) becomes altered, and that the change in the directions in which its atoms transpose and arrange themselves, determines the new arrangement of the sugar atoms, and the new positions which they assume, because it is not only by different temperatures that different products may be formed out of one and the same substance, but the same result is obtained if an alteration is made in the nature of the ferment transmitting its own action to the substance upon which it operates.

"Milk becomes sour from the sugar of milk which it contains being converted into lactic acid; this transformation caused by the presence and contact of the caseous matter, which is itself undergoing a state of change, of transposition of its atoms, from the influence of the atmospheric oxygen. If, after the entire disappearance of the milk-sugar, we add a new portion, the process of fermentation will continue as long as any cheese remains in the fluid to act upon the sugar.

"This manner of fermentation of milk-sugar without evolution of gas, is confined to the common temperature. At higher temperatures, as from 76 to 86 degrees, the products are totally different; the cheese assumes the properties of ordinary yeast, and two consecutive processes of transformation take place in the milk-sugar. In the first place, the sugar enters into chemical combination with a certain amount of water, and becomes converted into grape-sugar; that is, a sugar possessing precisely the same character as that found in grapes; and subsequently to this it is further transformed, while still in contact with the cheese, into alcohol and carbonic acid.

"Thus, whilst milk at common temperatures yields lactic acid as the principal product of the decomposition of its sugar, at a higher tempera-

ture we obtain as the product, an alcoholic fluid, which, upon distillation, furnishes a true brandy.

"If instead of yeast a small quantity of white cheese-curd is added to a solution of common sugar, and kept at a temperature of 76 to 86 degrees, and moreover, some chalk added to maintain the fluid in a neutral state, a lively evolution of gas soon takes place, the sugar totally disappears. Carbonic acid and hydrogen are given off in a gaseous state; and we find in the fluid a copious amount of *butyric acid*, one of the most interesting of the organic acids, which, until recently, was known only as a constituent of milk or butter.

"In ordinary fermentation the atom of sugar divides itself into two products, but in the fermentation I have just described three new substances are produced; in the former, alcohol and carbonic acid, in the latter, instead of these, we obtain, *butyric acid*, hydrogen gas, and carbonic acid. The relation of these substances to each other cannot be mistaken. Alcohol is *butyric acid hydrogen*. The atom of butyric acid is an alcohol atom, from which two atoms of hydrogen have separated.

"Thus, alterations in the nature of the products of fermentation ensue with every variation in the process induced by changes of temperature or the presence of matters accidentally drawn in to participate in the transformations. The same grape-juice, when fermented at various temperatures, yields wine of dissimilar qualities and nature, depending upon the circumstances of the temperature of the air being higher or lower during autumn, and according to the depth of the cellar in which the fermentation is conducted, which vary the quality, the odor, and the flavor of the wine. An uniform temperature of the place where the fermentation is conducted, ensuring its slow and gradual progress, are the principal conditions depending upon our own control for the production of the best kinds of wine. The growers of wine will soon universally give the preference to deep rocky cellars or vaults for conducting the process of fermentation; such vaults have been found particularly appropriate for the fabrication of the superior varieties of beer, and the advantages of these vaults mainly depend upon their constant temperature.

"The influence which extraneous substances exercise upon the products of vinous fermentation is strikingly exemplified in the fermentation of potato-mash. It is well known that in the manufacture of potato-spirit an oily liquid is obtained, besides the alcohol, possessing poisonous properties a highly disagreeable smell, and nauseous taste; this is called *fusel oil*. It does not exist ready formed in the potato, but is a product of the transformation of sugar, for it is produced not only in the fermented potato-mash, but also in the fermentation of the last syrups obtained during the fermentation of the beet-root sugar.

To be sure, Liebig is speaking rather of fermentation than putrefaction; but the difference is more in words than in reality. To use his own language, "putrefaction is the process of fermentation in organic substances containing nitrogen and sulphur, which give rise to the formation of products of a disagreeable odor."\*

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\*London Lancet, June 29, 1844.

It is obvious, therefore, that the putrefactive process may or may not give rise to the production of poisons, according to circumstances.

6. The next objection that occurs to me, is, that the meteorological conditions spoken of as being necessary to the production of the poison of yellow fever, only exist in our own imagination. The assumption of any such conditions is a pure hypothesis, unproved and unwarranted by any facts. Thus Hillary on the Diseases of Barbadoes :

“It does not appear from the most accurate observations of the variations of the weather, or any difference of the seasons which I have been able to make for several years past, that the fever is in any way caused or much influenced by them: for I have seen it at all seasons of the year, in the coolest as well as the hottest time of the year, except that I have always observed that the symptoms of this, (as well as most other fevers,) are generally more acute, and the fever usually higher in a very hot season, especially if it was preceded by warm moist weather, than it usually is when more cool.”\* Again, according to Imray, (from whose paper the above remarks are taken,) M. Rufz coincides with the opinions of Hillary. He says, “now whether the thermometer was high or low, whether it rained or was fine weather, hot or cool, yellow fever always prevailed with the same intensity, without the character of the seasons appearing in any way to influence its progress.”

Whatever it may be in Barbadoes, such is certainly not the case in New Orleans. The first frost sufficiently severe to kill the leaves of our forest trees, or our annual plants, puts a sudden and effectual stop to the disease as an epidemic. Its production and continuance must, therefore, depend on certain meteorological influences.

Again, the appearance of yellow fever in New Orleans is limited to a few months in the year. We never see an epidemic of it in winter or in the spring.

Again, when violent epidemics — such as those of 1837–39–41 — make their appearance in New Orleans, it is notorious that almost every Southern city is also afflicted with the disease. These facts appear to me to be conclusive with regard to the existence of some general epidemic influence.

It is no answer to tell us that we do not know what this influence consists in. Meteorology cannot pretend to the dignity of a science: its facts are too scattered, broken, unconnected.

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\* Observations on the Nature, Causes, and Treatment of Yellow Fever. By John Imray, of Dominica, M. D., etc. Edin. Med. and Surg. Journal, Oct. 1, 1845.



To overlook meteorological influences when epidemic diseases prevail simultaneously in different cities, within certain latitudes, seems to me as wise as to contend that effects may happen without causes.

7. But there is another objection, and one in my opinion by far the most fraught with mischief. It has a practical bearing, and is therefore the more dangerous. This consists in supposing that this meteorological influence is the *origo mali*;—the sole and only cause of the disease. If such be the fact our case is hopeless; for no human power can control the meteorological conditions of the earth or atmosphere, and we must, therefore, abandon forever all expectation of eradicating the disease from our city.

But such a gloomy view of the matter is not at all tenable. Unacclimated persons may live within a few miles of the city, and be perfectly safe from attack, provided they keep away. Persons arriving in the city from abroad are liable to attack as early as the second or third day. Now it is obvious that these facts are altogether opposed to the meteorological theory; without indeed it be contended that the meteorological change is confined to the city, which is an absurdity.

If yellow fever arose from a vicious constitution of the atmosphere, it would follow, that almost all persons liable to the disease and living in such an atmosphere, would be stricken at the same time, or nearly so. Making every allowance for more or less susceptibility, one could not live for any length of time in an atmosphere, in which another is poisoned.

The whole rise, course, and progress of our epidemics show, that yellow fever cannot arise from such general cause. Persons spend months in the very midst of it, to be taken down at last, just as the epidemic is disappearing. Its whole course seems rather to indicate, that it springs from points or *foci*, in which the poison is being continually formed during the existence of the meteorological conditions necessary for its formation; and these foci may be far apart.

In concluding this paper, I may sum up by saying, that in my opinion yellow fever does not depend on putrefaction alone, nor on meteorological influences alone, but on both conjoined; when this conjunction occurs, seething laboratories of poison are put in operation at different points of the city.

If such be the case, the remedy is within our own reach. Keep the city clean—remove all filth as soon as discovered, and let it be cast in the river. But if we really wish to derive a practical benefit, these operations



must be carried on upon a far greater scale than has ever heretofore been done. Sending a few scavenger carts about the streets, is but a poor and piddling affair. A board of health *with police powers* should be created, whose duty should be to inspect private yards and lots—to see that the owners filled them up—that the privies were emptied at least once in three months,—and particularly that no nuisance should be permitted to lie about the levee. Besides all this, the streets should be paved, efficient water-works constructed, and the back of the city thoroughly drained. Running water will keep sweet—or if foul, when commencing to run, will become sweet by being put in motion. This fact was particularly called to my attention recently by the very able engineer of the draining company. The water of the gutters, foul in the extreme, towards the upper part of Common street, was comparatively purified before it reached Claiborne street. The fact needs no commentary.

New York, Philadelphia, and Baltimore have now by their wise police regulations enjoyed immunity from yellow fever for many years. New Orleans may do the same if she has energy to put her shoulders to the wheel.

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## Interesting Cases:

*Reported by P. O. TETE, Student, Charity Hospital.*

William Langshaw, aged 24 years, Native of Ireland, was brought to the Charity Hospital in a state of insensibility on the night of the 8th of June, 1855.

L. was working on board of a ship and by accident fell into the hold, which was of considerable depth, receiving a very severe contused and lacerated wound of the scalp. On a thorough examination no fracture was detected, and the wound was simply dressed with adhesive strips, drawing the flaps together. Patient at this time laboring under violent concussion of the brain.

The next morning he was sensible, but had entirely lost his voice and hearing. Being otherwise pretty well, he took no medicine except a calomel purge.

On the 10th there was strong reaction; pulse full and frequent, face

flushed and complaint of excessive pains in the head and epigastrium. There were evidently all the symptoms of inflammation and, consequently, the antiphlogistic treatment was adopted. He was bled to the amount of about fifteen ounces, and ordered the following :

Hydrarg. chlorid. mit.....gr. xxiv.  
Divide in cht. duodecim.  
S. One every hour.

June 11th — Patient hears a little better, but can speak only in a whisper. Still complains of pains in the head and epigastrium. Not being under the influence of mercury the same quantity of calomel was again ordered to be taken in the same manner, and cups were applied to the epigastrium to relieve pains.

June 12th — Patient feels better. He was ordered an ounce and a half of castor oil, and cold applications to head. Being unable to adjust the flaps of the wound with adhesive plaster, and by order of ward surgeon, I put in three sutures, which served the purpose very well.

June 13th and 14th — Patient is doing well. Hears better, but is in the same state of aphonia. Continued cold applications to head.

June 15th — Patient having a slight fever was ordered six grains of quinine and Dover's powder. From this day his general condition and hearing improved, but he could speak only in a whisper until the 29th when there was a decided change in the voice.

July 6th — Since the 29th patient's voice has gradually improved and is now perfectly natural. The wound of the scalp is also improving and will be healed in a few days.

This case is particularly interesting on account of the entire suspension of voice and hearing.

Wm. McCauley, aged 35 years, entered the Charity Hospital on the 12th of June, 1855, with a penetrating wound of the abdomen, about three inches in length, immediately below the ensiform cartilage. It was inflicted by himself with a dull pocket knife. The wound was dressed with a few strips of adhesive plaster and patient was ordered half an ounce of syrup of morphine.

About one hour after I was called by the nurse and found patient suffering very much, and by straining he had caused part of the stomach to protrude through the wound. After endeavoring for ten or fifteen minutes to push it back into the cavity, it was found impossible on account of the patient straining so much. Chloroform was then administered, and scarcely

had he inhaled it two or three times when the stomach was pushed back to its natural position without any difficulty. Three sutures then served to close the wound, after which a thick compress was placed over it and then a bandage about six inches wide was passed around the body to hold the compress in place. Patient was also ordered twelve grains of calomel divided into twelve powders—one every hour.

Two days afterwards the sutures were withdrawn, but adhesion of the lips of the wound had not taken place. However, the stomach having become agglutinated to the walls of the abdomen, closed the opening, through which, as I omitted to mention above, part of the left lobe of the liver could plainly be seen.

The position of the wound occasioned great difficulty in adjusting its lips, and after having tried several ways, the following, proposed by the ward surgeon, was found to be the best: Strips of adhesive plaster about two inches wide and eighteen long were drawn obliquely across the wound from the left side of the thorax to the right lumbar region.

For about one week patient had constantly a little fever and was very low spirited. The fever, however, gradually subsided, and he has since continued improving. He is out of bed since the 28th of June, and will soon be well enough to be discharged.

This case illustrates most beautifully those happy efforts of nature which so frequently come to the relief of the surgeon. The first blaze of inflammation (peritonitis) had the effect of sealing the opening into the cavity of the abdomen, the peritoneal surfaces of the stomach and walls of the abdomen being brought in immediate contact; and in this lay the only hope of the patient.

A CASE OF TRAUMATIC TETANUS.—Catharine Condon, aged 18 years, native of Ireland, admitted into the Charity Hospital on the evening of the 18th of June, 1855. One week previous to admission, patient stuck a splinter in her foot, which, as she stated, had been extracted immediately. She suffered but little after this until the morning of the 17th, when she felt a stiffness about the muscles of the neck, swallowed with difficulty and could open her mouth but partially. There was also a rigidity of the sterno-mastoid muscles. On the evening of her admission she was ordered an ounce and a half of the *ol. ricini* and the following:

R	Ext. hyosciami.....	gr. j.
	Ext. opii.....	} aa gr. ss. ℥.
	Ext. belladonnæ .....	
	Ft. pilula.	

To be taken after operation of cathartic. Also, the following liniment :

R Liniment saponis..... ℥iv.  
 Chloroformi..... } aa ℥ij. ℥.  
 Tinct. belladonnæ..... }

To be rubbed along the spine and neck.

June 19th—Patient rested a little last night, opens her mouth better, but has a violent pain in her back, and she is slightly curved backwards.

The Visiting Surgeon of the ward saw her and ordered the following :

R Quiniae sulphatis..... } aa ℥j.  
 Pulv. camphoræ..... }  
 Morphiae sulphatis..... gr. ij.  
 Chloroformi..... ℥ij.  
 Mucil. g. acaciæ..... ℥iv. ℥.

A tablespoonful to be taken every half hour until she sleeps.

Night—Patient slept a little during the day, but is not better; continued mixture and ordered a purgative enema.

June 20th—Patient is worse and opisthotonos complete. The Surgeon seeing some indications of a foreign substance in the foot, put the patient under the effect of chloroform, and having made a free incision, extracted a splinter about one inch in length, which was lodged about two inches from the wound, in the direction of the external malleolus. He then cauterized the wound with lunar caustic, and ordered a poultice to be placed over it. He also ordered brandy freely, the same mixture to be continued and chloroform to be given by inhalation several times during the day, in order to relieve the pain and procure sleep. Patient was removed from the ward and placed in a dark retired room which was more suitable to her condition.

Night—Patient no better; administered chloroform by inhalation and continued treatment.

June 21st—Patient has frequent spasmodic contractions of the muscles of the back and suffers exceedingly. Cups were ordered to be applied along the spine, one drop of the ol. tigllii, and the mixture to be continued with an increased quantity of morphine and chloroform.

Noon—Administered the chloroform by inhalation for the fourth time to-day. Patient no better.

Night, 9 o'clock—Patient very weak; pulse feeble; extremities cold. At her own request I gave her the chloroform again, with the intention only of relieving her of the pains, for at this period no other benefit could be expected. She expired two hours after in a spasm. In this case very little chloroform would suffice to put the patient into a quiet sleep. and it



gave her so much relief that every time she saw me she begged me to give it to her.

The Surgeon's intention in this case was to perform the operation of tracheotomy, in order to relieve the spasms. But at his last visit not finding that she had much difficulty in breathing, he deferred the operation, and requested the House Surgeon to perform it, should it be necessary. However, as there seemed to be but little difficulty in breathing during the day the operation was not performed and the patient died, as stated above, in a spasm.

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## Penetrating Wound of the Bladder:

By W. H. BERTHELOT, Student, Charity Hospital.

Charles Jenkins, native of Ireland, aged 22 years, of plethoric habit, entered Charity Hospital on the evening of 27th June. Two wounds, inflicted with a knife, were found on his person, one of which was over the inferior portion of the left scapula, the other in the left lumbar region, about two inches from the spine. Neither wound penetrated to any considerable depth. They were dressed with adhesive straps, and half an ounce of syrup morph. given to the patient. He slept well that night and was doing well next morning. Was ordered lemonade and half diet of the Hospital.

Next day (June 29th) patient complained of great pain in the hypogastric region, and examination revealed a small wound situated immediately over the crest of the pubis, bearing a direction downwards and backwards and *penetrating the bladder*. Urine escaped through the wound, and that which passed through the urethra was voided with great difficulty and was tinged with blood. The lower part of the abdomen was very tender to the touch—pulse full and rapid.

TREAT.—Venesection to 14 ounces, 2 grains calomel and  $\frac{1}{2}$  grain opium every hour.

At 5 P. M. peritonitis seemed to be extending—abdomen was tender over the whole surface—countenance expressive of anxiety—complained of the weight of the bed-clothes—knees drawn upwards—bled him from the arm to fifteen ounces, with great relief. A light flaxseed poultice with tinct.

belladon and tinct. aconite was applied over the abdomen, after which he slept a little. Calomel and opium continued.

June 30th, pain considerably relieved—pulse 90—gums being slightly touched calomel was discontinued—poultices continued.

[July 1st, wound is closed and abdominal tenderness subsiding. Ordered Dover's powders.

From this time patient improved steadily, with the exception of slight interruption from moderate diarrhea, and on July 19th he left the Hospital entirely well.

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## A Case of Stricture of the Urethra.

### SUCCESSFUL OPERATION.

*Reported by E. D. CHENEY, M. D.*

Mr. T. had been laboring under stricture of the urethra about ten years, induced by a protracted attack of gonorrhea, under the erroneous impression that he was afflicted with urinary calculus; he had been treated with various quack remedies without benefit. When I first saw him he was suffering from retention of urine, the fluid passing in a small quantity, and he was gradually sinking from the accumulation of urea in the blood, accompanied with the unpleasant symptoms usually manifested in such cases. After several ineffectual attempts to relieve the stricture with dilating instruments, etc., it was determined, on consultation, to operate.

The patient was placed in position for the operation of lithotomy; the perinæum was shaved and a silver catheter was afterwards introduced into the urethra as far as the stricture. An incision was then made posterior to the scrotum down to the point of the catheter. A small grooved catheter was then substituted, and an attempt made to pass the stricture, but the coarctation was so great that the effort was unavailing.

The stricture was now freely divided with a probe pointed bistoury. The stricture involved about two inches of the urethra, and the tissues proving to be very much thickened and almost cartilaginous, a silver catheter was introduced and allowed to remain twenty-four hours, this being deemed sufficient time for the formation of coagulable lymph. An ineffectual attempt was made to promote union by the first intention by means o

adhesive strips and sutures, but the urine, notwithstanding, passed through the opening in the perinæum for three weeks.

During the entire treatment, which lasted several months, it was necessary to use catheters to prevent a removal of the stricture, and this course was continued, *pro re nata*, for six months. The patient's health was finally restored, and up to the present time, (eighteen months since the operation,) there has been no return of the stricture.

An operation for a stricture of this character, not complicated with abscess, infiltration of urine, or fistula, is very rarely necessary, but I conceive that in this case the most skillful surgeon could not have succeeded in relieving the stricture by the simpler process of dilatation.

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## Remarks on Yellow Fever --- No. 2.

By E. D. FENNER, M. D.

*Messrs. Editors:* — In the last number of your journal I gave you some remarks on the mode of origin and the manner in which yellow fever usually commences its ravages and runs its course in this city, which I shall now endeavor to substantiate by statistical facts obtained from the Charity Hospital.

I stated in the first place, that yellow fever does not burst out suddenly here, presenting characteristic features different from all other fevers, and therefore easily distinguishable; but, on the contrary, that the endemic fevers (bilious remittent and intermittent) gradually run into this type, and assume its features in such a manner as to greatly embarrass the diagnosis previous to the termination of some cases in black vomit or other hemorrhage. And farther, that at the same time might be seen numerous cases of bilious remittent and intermittent fevers.

Now let us see what the records of the hospital show, and to illustrate the points I shall select from a few prominent epidemic and non-epidemic years.

In 1841 there were admitted into the Charity Hospital 2,057 cases of fever; of which 1,113 were yellow fever, 800 intermittent and 99 bilious remittent fevers.

The first case of yellow fever occurred on the 9th of June, and no other case was admitted till the 2d of July. During this latter month twelve cases of yellow fever were admitted, and there were many cases of intermittent and remittent fevers. In August yellow fever became epidemic. The following will show the relative proportion of the three types of fever under consideration, for four months:

<i>Fevers.</i>	<i>Intermittent.</i>	<i>Remittent.</i>	<i>Yellow Fever.</i>
August .....	151	31	169
September .....	18	5	642
October .....	66	1	252
November .....	93	7	37
Total.....	328	44	1,100

A few cases of yellow fever were admitted in December. Here we see the first appearance and gradual rise of yellow fever in the midst of intermittent and remittent fevers, the two latter types combined reaching to one-third of the amount of the former during its epidemic prevalence.

In 1843, another epidemic year, there were admitted of all kinds of fever 2,222 cases; of which 1,053 were yellow fever, 843 intermittent, and 276 remittent bilious fevers.

Yellow fever prevailed in the months of July, August, September, October, November and December; and the following will show the relative proportion of intermittent and remittent fevers at the same time:

<i>Fevers.</i>	<i>Intermittent.</i>	<i>Remittent.</i>	<i>Yellow Fever.</i>
July .....	70	77	23
August.....	98	80	188
September.....	128	51	365
October .....	136	14	351
November .....	149	11	111
December.....	76	12	15
Total.....	657	245	1,053

We have here the astonishing fact, that whilst yellow fever was prevailing, there were almost as many cases of intermittent and remittent fevers combined, 902 — yellow fever, 1,053.

In 1847, another severe epidemic year, there were admitted 6,901 cases of fever of all kinds; of which 2,811 were yellow fever, 2,192 intermittent, and 311 bilious remittent. Yellow fever prevailed from June to January, and the following will show the relative proportion of intermittent and remittent fevers at the same time:



<i>Fevers.</i>	<i>Intermittent.</i>	<i>Remittent Bilious.</i>	<i>Yellow Fever.</i>
June.....	211	42	5
July.....	223	76	148
August.....	74	66	1,611
September.....	53	26	777
October.....	258	21	219
November.....	380	25	49
December.....	341	16	2
Total.....	1,540	272	2,811

Here again we see more than half as many cases of intermittent and remittent fevers as there were of yellow fever at the time of its prevalence. (Intermittent and remittent fevers, 1,812; yellow fever, 2,811.)

We come now to the great epidemic year 1853. The total number of fever cases admitted into the Charity Hospital this year was 6,898 of which 3,217 were yellow fever, 2,640 intermittent, and 442 remittent fever. There were five cases of yellow fever admitted in May and five in December, but this disease may be said to have *prevailed* from June to December. The following is a monthly statement of the three types under consideration during this time :

<i>Fevers.</i>	<i>Intermittent.</i>	<i>Remittent.</i>	<i>Yellow Fever.</i>
June.....	314	79	50
July.....	335	126	1,157
August.....	181	15	1,495
September.....	112	21	350
October.....	229	34	142
November.....	237	12	14
Total.....	1,408	287	3,208

Thus it will be seen that the intermittent and remittent fevers combined (1,695) amount to more than half the number of yellow fever cases during the severest epidemic of the latter ever witnessed in New Orleans.

So much for the statistics of fever in seasons *decidedly epidemic*. They certainly show the first appearance or outbreak of yellow fever in the midst of intermittent and remittent bilious fevers, the gradual ascendancy it gains over these types of fever up to a period when it reaches a *climax*, from which it again gradually declines, and finally gives place to the ordinary endemic fevers. At this latter period we often meet with the same difficulty in deciding whether a case is yellow fever or bilious remittent that occurs at the outbreak.

Let us now see how the thing goes in *non-epidemic seasons* :

In 1842 the total number of fever cases admitted into the Hospital was 1,758; of which there were of intermittents 1,092, of remittent bilious

178, and of yellow fever 410. Yellow fever was seen in August, September, October and November, accompanied by the other types as follows:—

<i>Fevers.</i>	<i>Intermittent.</i>	<i>Remittent.</i>	<i>Yellow Fever.</i>
August.....	169	37	47
September .....	144	42	247
October .....	140	37	93
November .....	110	13	23
Total.....	563	129	410

Here we see the great preponderance of intermittent and remittent fevers in a healthy year.

In 1844 the total number of admissions for fevers was 2,207; of which there were intermittents 1,589, remittents 257, yellow fever 152. The yellow fever of this year commenced in June and continued till November, as follows:

<i>Fevers.</i>	<i>Intermittent.</i>	<i>Remittent.</i>	<i>Yellow Fever.</i>
June.....	75	24	1
July .....	166	30	1
August.....	258	47	1
September .....	255	67	68
October.....	261	55	52
November .....	216	5	25
Total.....	1,231	228	148

These figures speak for themselves.

In 1845, there was admitted *but one single case of yellow fever*, and that was in February. It was the healthiest year I have ever known in New Orleans. The total number of fever cases admitted, was 1,763; of which, there were, intermittents 1,403, remittents 168.

In 1846, the total fever admissions were 2,603; of which, there were intermittents 2,078, remittents 107, yellow fever 148. Yellow fever was seen from September to December, as follows:

<i>Fevers.</i>	<i>Intermittent.</i>	<i>Remittent.</i>	<i>Yellow Fever.</i>
September.....	359	24	29
October .....	376	36	83
November .....	310	7	32
December ... ..	81	3	4
	1,126	70	148

These statistics of fever in *non-epidemic years* I deem sufficient to substantiate my remark, that “if the season turns out to be *not very sickly*, the yellow fever type will not gain the ascendancy over all others, but only appears sporadically and to a moderate extent.”

As to my position that the endemic fevers of this place gradually run into yellow fever, in fact, that the latter is only one of the types of endemic

fever, common to this region, and *not a separate, distinct, and specific* disease, if it is not sufficiently established by the foregoing statistics, I can give cases in which the disease commenced as an intermittent, afterwards presented the full livery of yellow fever, and finally terminated in the intermittent type again.\* The very fact annually witnessed in this city of the difficulty of determining when yellow fever actually commences, affords powerful evidence of the intimate relationship that exists between intermittent, remittent, and yellow fevers. The celebrated Dr. Chervin, who studied yellow fever in different parts of the world, more thoroughly than any man that ever lived, has well observed — “if a light remittent fever be compared with a very intense yellow fever, we shall, without doubt, see very marked differences in the symptoms of the two affections; but, if we put beside a severe remittent, a mild case of yellow fever, we shall see none, for there is a point where these fevers are so confounded, that they really become one and the same disease — the same affection under different forms and various degrees.”

The *Annual Report of the Board of Health on the Sanitary Condition of the City of New Orleans for 1848*, drawn up by a committee consisting of the late Dr. W. P. Hort, Dr. J. J. Ker, and Dr. Y. R. Lemonnier, took special notice of my “*Fever Statistics*,” published in the 5th vol., No. 1, N. O. Med. & Surg. Journal, and remark upon them as follows: “Three inferences are drawn from these facts:—*First*, That when the intermittents predominate to a considerable extent throughout the summer and fall months over the yellow fever cases, as in 1844 and 1846, the disease (yellow fever) may be set down as sporadic.

“*Second*, When in August and September the intermittents suddenly decreased with large preponderance of yellow fever, as in 1847, then there can be no doubt of the existence of an epidemic; which was the case in 1841 and 1843.

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\* We have always conceived the position here taken by Dr. Fenner, to be the proper one. In the beginning of every season, the observer must be struck with the fact, that physicians are at a loss to diagnose the earliest cases of yellow fever which enter the Hospital; and it is not unfrequently the case that *black vomit* is the first thing which arouses suspicion; the cases are diagnosed “remittent,” and treated as such, until this symptom appears. After the first few cases of yellow fever, the diagnosis becomes an easy task, as all remittents, and even some intermittents, are decidedly prone to run into black vomit. We have seen not a few cases of fever die in the Hospital this season with black vomit, and yet, present no yellowness of skin or conjunctiva previous to death. As the cases increase, however, the fever-cause being apparently more potent, the symptoms are more decidedly marked, (they are graver,) and the cases are diagnosed more readily.

"*Third*, That the intermittents, never being entirely suppressed during the climax of the epidemic yellow fever, and increasing so rapidly as the epidemic declines, considerable affinity would appear to exist between the two descriptions of fever; which, if true, would forever settle the question of quarantine in the case of yellow fever."

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## Editorial and Miscellaneous.

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### TRANSACTIONS OF THE MEDICAL ASSOCIATION OF THE STATE OF ALABAMA.

We have been presented by a friend with a copy of the "Transactions of the Medical Association of the State of Alabama, at its Eighth Annual Session, begun and held in the city of Mobile, February 5-6-7, 1855: together with the Code of Medical Ethics and a List of Members."

It is with mingled feelings of pride and humiliation that we peruse the pages of these transactions — pride that the medical men of our sister State (Alabama) have done so much honor to themselves and to science; humiliation, that Louisiana, so far from being able to offer in exchange a like valuable repertory of medical knowledge, must confess to such an utter want of public spirit, that her State Medical Society, founded a few years ago, has utterly failed to bear well matured fruit. We do not stop here to inquire into the causes of this unpardonable delinquency; they may be readily ascertained by all who will honestly question themselves on the subject.

First on the list of valuable contributions to the Transactions, are two able reports on the Medical Botany of Sumter and Dallas counties, the former by Dr. S. W. Clanton, the latter by Dr. P. H. Cabell. These gentlemen deserve great praise for their industry.

Dr. J. W. Crawford, in his "Report on the Diseases of Centreville and vicinity," makes some interesting, though rather desultory, remarks on several affections. He mentions nineteen cases of typhoid fever treated on one farm situated on "low flat lands" between two creeks; of these, sixteen were attended with "bronchial irritation sufficient to demand special attention." That this species of irritation is a constant accompaniment of the continued or typhoid fevers (more especially among negroes) is our experience, though we have rarely found this symptom so prominent as to demand remedies especially directed to its control; it has nearly always appeared intercurrent and unimportant, though we can readily conceive of its becoming prominent and dangerous; the protean proclivity of epidemic diseases is but too well known to medical men. Dr. C. also mentions diarrhea as an inva-



riable symptom in these cases, and says he treated the same satisfactorily with acetate of lead. He also mentions the occurrence of hemorrhage from the bowels in three of the cases. Of the nineteen but one died, for which success Dr. C. is entitled to great credit; only the initiated can form a just estimate of the difficulties to be encountered in the treatment of epidemic diseases amongst negroes.

Dr. C. mentions, too, a number of cases of pneumonia, many of which, assuming the typhoid form, were very troublesome to manage, and several died (pity the Dr. had not been more explicit in his statistics concerning this most formidable and far too prevalent disease). He says, the treatment "consisted in calomel and Dover's powder, in small and frequently repeated doses, tartar emetic and blisters." Also, "I have watched anxiously and as carefully as I was competent to do, for a remission, with a desire to give quinine a trial; but in none of the cases could I detect such a remission, either by the pulse, a decline in the pain, or heat of the surface, or otherwise, consequently I have not used it in the treatment of these cases." That calomel, Dover's powder and blisters are good remedies in certain cases of typhoid pneumonia we can testify; but these remedies must be confined to the higher, or, indeed, *highest* grade of the disease; in the lowest form of asthenic pneumonia amongst negroes, we have been taught by sad experience to withhold them, more especially the calomel and blisters—the former often exciting unmanageable diarrhea, and the latter increasing the prostration through the means of pain and strangury. In some few cases the Dover's powder acted very well as an anodyne and sudorific.

With regard to quinine, our experience has taught us that it is useful in but one way in typhoid pneumonia, and that is as a permanent tonic; given with this view, it is the most reliable remedy we have ever tried, but it can meet no other indication in the disease. In administering it thus, we have paid no attention to remissions; in the true asthenic form of the disease the indication is to support the failing powers while nature relieves herself, and quinine answers this purpose better than any remedy we have yet tried, though we claim for it no *specific* effect; we know of no remedy which can be said to have anything approaching a specific control over asthenic pneumonia; the disease will run its course in spite of everything, and if we can only avert the general tendency to death we must be content.

As for Dr. C.'s remaining remedy—tartar emetic—in the epidemics we have witnessed, it has proven absolutely poisonous; we never saw it beneficial in other than frank asthenic pneumonia, and even in these cases (amongst negroes) we have seen its too free administration *produce* typhoid symptoms.

Dr. C. mentions the fact that whooping-cough had been prevailing in his neighborhood pretty extensively, but says that he had been called on to interfere in a single case only. We congratulate him on having escaped experience in this disease in its malignant epidemic form; we do not know a more serious or more troublesome disease as we have witnessed it amongst little negroes.

Dr. E. P. Gaines reports a very interesting case of gun-shot wound of the lungs resulting in recovery.

Dr. J. C. Nott, of Mobile, offers some very interesting "remarks" on a few points in surgery, and presented the association an instrument of his own inven-

tion for excision of the tonsils. It is a matter of regret that Dr. Nott does not extend his "remarks" on surgery in our journals. Southern surgery has not been awarded the position to which it is entitled, and it is simply because our surgeons are too sparing of their ink; we know of no one better calculated to place us aright than Dr. N., and we are sure that his silence is not because he loves surgery less, but that he loves other subjects more.

Dr. W. Taylor, also, contributes a "Report on Surgery" for Talladega county. The cases appear to have been treated by Dr. J. C. Knox, of that county, during the past twelve years. Amongst the cases is one of "reduced luxation of the hip joint after six weeks standing." Also, "reduced luxation of the radius of three months standing." It is much to be regretted that the above is all that is said on the subject of these luxations; it is not even said which end of the radius was dislocated. Dr. K. also mentions two cases of ruptured uterus, but does not even say whether any attempt was made to save the children.

Dr. Groce reports a successful operation for stone, the incision having been made "through the vagina and urethra, about one and a half inches from the external meatus." Also, two cases of unsuccessful Cæsarean section, in each of which the fœtus was found in the cavity of the abdomen. He appears to have regarded them as cases of extra-uterine fœtation, until the autopsies revealed rupture of the uterus. In one case the *placenta* was found in the cavity of the abdomen, the uterus being extensively lacerated.

Dr. F. E. Gordon contributes a "paper on the treatment of Dysentery." He seems to be still writhing under the rather unkind strictures sometime since directed against "South Alabama physicians" by one of the fraternity in our city. Dr. G. must recollect, however, that there are two opinions on this subject in New Orleans — that the South Alabama physicians do not meet with universal condemnation here. His treatment of dysentery appears most reasonable — all but the injections of nitrate of silver; if the rectum be the sole seat of disease, we might conceive of this local application being reliable, but dysentery is not confined to the rectum. Having never tried the injections recommended, of course we only theorize on the subject.

Dr. Lopez, of Mobile, reports three very interesting cases of "Metastasis of Disease" — two of them subjects of leucorrhea, in whom, with the subsidence of the discharge, they were seized, one with rheumatic symptoms, the other with gastralgia. Our observation satisfies us that such cases are more common than is generally supposed.

The "reports" of Dr. C. Troy, of Cahaba, and of Dr. W. Taylor, of Talladega, are replete with valuable information, and we regret that our limited space denies us the pleasure of noticing them *in extenso*. These reports, as well as others in the Transactions, should have a more extensive circulation amongst the profession than is given them by the association.

Last, though not least, on the list of reports, is that of Dr. G. A. Ketchum, "on the Diseases of Mobile for 1854." Dr. Ketchum is one of our "working men," and it is a matter of regret that he does not oftener shed his ink for the benefit of Science through the medium of our medical journals.

Dr. K.'s remarks about pthisis leads us at once to echo the idea he advances in relation to the effect of our climate on this class of persons "who visit us from a distance." We have long been satisfied that the instances of real benefit derived by these patients are "few and far between," while the great majority come here to die, or, at best, to return infinitely worse than when they came. It is very strange that in the face of the formidable array of unsatisfactory results which are annually made evident, our Northern and Western brethren will send their pthisical patients here.

Dr. K. mentions a sort of epidemic of colic which prevailed in Mobile during June and July, 1854, and arrives at the very sensible conclusion that "many of these cases, if not all of them, resulted from lead poisoning." This would not require a great stretch of the imagination, when we are informed that "the water furnished the citizens of certain portions of Mobile, is carried through *eight miles of main pipes of lead*." Is it not "passing strange" how little attention is paid by our people to health? There is not one of these same individuals who would not immediately resist any attack on his pocket, and yet he is content to drink a slow poison from year's end to year's end, because it is a matter which concerns the *public*; if his neighbor can drink the water, he can. We hope Dr. K. will not drop this subject here; he has only commenced the work for Mobile; he has not carried his investigations as far as Dr. Fenner, of this city, did a few years ago—a gentleman to whom is due all the credit of having detected the true nature of a similar epidemic here, and for having at least established the fact that *one* prolific source of the affection is to be found in the numerous soda founts, the water of which is almost invariably impregnated with lead derived from the pipes thereto attached—the extent of impregnation varying with the draught made by customers on the founts.

Dr. K. also mentions the occurrence of an epidemic of "dengue fever" in July, 1854, and we are rather inclined to infer that he leans to the opinion, beginning to be not unfrequently expressed, that it (the dengue) is neither more nor less than a mild form of yellow fever; though we would not be understood as wishing at all to compromise the Dr. to this doctrine; we only draw our inference from the following expression:

"Towards the close of the month I saw many cases of a malignant type, a cross, as it were, between dengue and yellow fever. The attacks set in usually with the more marked symptoms of dengue, but gradually assumed the appearance and symptoms of the more malignant disease towards its close; these cases then required all the careful watching and management of regular yellow fever attacks."

Dr. K. says that yellow fever was not epidemic in Mobile in 1854; he is convinced that "as yellow fever usually appears, it is not an infectious or communicable disease," and he believes "that the abortive treatment, that is the judicious use of quinine early in the attack, is the treatment *par excellence*." Moreover, he says: "Exceptions must occasionally be made in the use of this remedy of course, but it will be found admirably adapted to a large proportion of attacks." He also advises the use of other remedies, as purgatives, baths, etc. If those who have *abused* quinine so much of late years, by pouring it down the throats of the sick



indiscriminately, *empirically*, will but keep the above remarks continually in mind, they will not only meet with better results in their own practice, but they will do much for humanity by restoring to its proper exalted position one of the most valuable remedial agents in yellow fever of which our *materia medica* can boast. It is the empirics, the one-idea routinists, on whom rests the sin of having brought this remedy into disrepute.

Dr. K. is a non-contagionist and an anti-quarantine man; he believes firmly in the local origin of yellow fever, and thinks Mobile, in her existing condition, quite as capable of originating a case of the disease as is any other place. He is a strong advocate for cleansing his city to prevent the origination of yellow fever. We are very sorry that we cannot notice this report more fully, but limited space compels us to stop here. Again we commend the perusal of these "Transactions" to all who can procure a copy. If the people of Alabama do not extend the hand of protection to medical men who labor thus, then they deserve to have yellow fever, and no quinine with which to treat it.

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#### YELLOW FEVER.

In the June and July numbers of our journal we reported all the genuine cases of yellow fever (eleven in number) which had occurred in the Charity Hospital up to June 25th. Our readers may rely on the data furnished, inasmuch as we promptly rejected all cases in relation to which there was any real doubt as to the nature of the disease, and we have thus been able to furnish them the most ample and reliable material on which to exercise their speculative propensities—more complete and more reliable than can be found elsewhere, as we recorded our notes at the bedside of the patients, and accepted nothing second-handed.

Just at this time, when the great question of quarantine is agitating the public mind, the most interesting question connected with these cases is their *origin*, and in reviewing them we shall merely advert to facts, without intruding our individual opinions. Every intelligent mind will make its own inference at last, and to afford the material for this purpose is our peculiar province.

In our June number is reported the case of John Hailey, who entered the Hospital on April 30th and died on 3d May. He seems to have been taken sick on his way to the city on board a steamboat, (either the *Louisa* or *Rapides*) on which he acted as cook. He was a native of Ireland and unacclimated. Yellow fever was not suspected at the time of his illness, and he was considered a case of "bilious fever" until black vomit made its appearance. No autopsy ever revealed more clearly all the characteristics of genuine yellow fever. Although the evidence is not *positive*, still, all the data tend to show that the individual had no communication with the shipping; he was taken sick away from the city, though he was running to and from the same every ten or fifteen days on one of the boats named.

The excitement caused by the case of Hailey in the Hospital soon subsided; none of the patients in the ward where he died took the disease, and things went on quietly until the 19th June, when a very severe case entered ward 22, and died on the 20th, after having thrown up black vomit profusely. This man's name



was Wm. Barneman — German — aged 21 years — one month in New Orleans — from Girod street — worked on the levee for steamboats — taken sick on the 15th June. Autopsy revealed plenty of pure black vomit in the stomach.

However, by reference to our reports in July number, it will be seen that a case of yellow fever, Carl Draugod, really appeared in the Hospital as early as the 15th June. He was seen by the visiting physician of the ward for the first time on the morning of the 16th, and although suspected at the time, nothing was said about it until the morning of the 19th, when he was bleeding freely from the nose — having, also, bled from the gums quite freely on the 18th, or the fifth day of the disease. All who saw this case pronounced it yellow fever.

Thus, then, the man Draugod (of June 15th) was the first case that appeared in the Hospital after the case of May 3d, or forty-three days previously. Let us see who Draugod is:—

He is a native of Germany, from Liverpool *direct*, and had been in this city five weeks when taken sick — had been acting as servant in the beer saloon of Frank Weber\* ever since his arrival, and had no connection with the ships. During this man's illness in the ward sundry cases of intermittent and remittent fever, diarrhea, etc., have occupied the beds around him, but none took yellow fever.

There is no evidence, then, of Draugod's having "caught" the disease from any person or bale of goods, etc.: he was not only the first recorded case for the month of June, 1855, but he was simply an unacclimated German, thrown amongst us at the beginning of our summer season, and notwithstanding he worked within doors and was not exposed to the vicissitudes of the weather, we find him taking yellow fever in its hemorrhagic form at a very early date — he having been sick two days before he entered the Hospital.

We next come to the case of Andreas Benz — German — in America three years — in New Orleans three weeks — last from Louisville, Ky. This man is a carpenter by trade, and was last at work near St. Mary's market — entered ward 23 on 19th June — had been sick with diarrhea nineteen days, and entered the ward with fever and diarrhea. Died June 21st.

On the same day that Benz entered (June 19th) John Kramer entered ward 24. This man was a German — aged 23 years — five years in America — six months in New Orleans — common laborer, but had been working in Murphy's Hotel, on St. Charles street, (next door to the St. Charles Theatre) for two weeks previous to being taken sick. He distinctly avowed that he had no communication with the shipping. Recovered.

These two individuals, then, (Benz and Kramer) are found entering the Hospital on the same day — one from a carpenter's shop near St. Mary's market, (one of the filthiest portions of the city) the other from St. Charles street, in the very heart of the city, and where most attention is paid to cleanliness. Both were strictly unacclimated; one in the city but three weeks, the other six months.

Next comes Edward Tilden — Irishman — has been in America nine years, but

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\*The patient being a stranger in the city, could not tell us the name of the street on which Weber keeps his saloon, though he says it is not near the shipping: and we have failed to find it in the City Directory.

has never resided in New Orleans—has been running to and from the city on steamboats for several years—entered ward 25 on June 20th, having been taken sick four days previously (16th)—was taken sick on his boat and went thence to a boarding house on Levee street. It is thus seen that Tilden was one of the first taken sick, he being only three days behind our first patient, Draugod.

Three days now elapse, and on the 23d June we find the man Philip Lyskchyler entering ward 22, with an intense attack of yellow fever, which terminates fatally on the morning of the 24th. He is a German—*direct* from Havre, on ship Rome,\* and has been in New Orleans but three weeks—has been working on the levee, near post 24—taken sick three days before he came in, or on the 20th June.

Next comes George Weintz—German—entered ward 22 on the 20th June. He is, also, from ship Rome, (emigrant) and says he knew Lyskchyler well—has been sick twenty-four hours—is from a boarding house near St. Mary's market—has been doing nothing since his arrival in this city—merely lounging about his boarding house. Recovered.

These two men (Lyskchyler and Weintz) were, also, wholly unacclimated, having been in New Orleans but three weeks. The former was laboring on the levee in the sun, the other was under shelter and doing nothing; yet both took the disease.

We now come to Maria Shualtz, who was brought to the Hospital in a hopeless condition on the morning of 25th June. She could give no account of herself, but her friend says she is a native of Germany—has been in New Orleans eight months—is from the corner of Claiborne and Poet streets, (Third District, and far back from the river) and was taken sick on 22d June. This patient came from a locality far removed from any of the other cases, and a great distance back from the river, and, consequently, from the shipping.

Lastly, we have the man Joel Shoemaker—native of Ohio—in New Orleans six months, though recently returned from a visit to Ohio—lives in Lafayette, at a hotel near the Stock Landing—entered ward on afternoon of 25th June and died 2d July. This man and Maria Shualtz entered the Hospital on the same day—both intensely attacked—yet coming from opposite ends of the city, and one from the river bank, the other from the rear of the city.

The data being now furnished, it remains for those who preach the doctrine of the importation of yellow fever into New Orleans to make out their case. We cannot close, however, without annexing a summary of two cases which we regard as closely allied to the subject: they will speak for themselves:—

June 15th—Pan Mordin—Swede—entered ward 17—he is a clerk—last from Chicago—five months in New Orleans. This man was entirely delirious, and presented all the symptoms of delirium tremens—fever arose on the day after his admission, and on the 19th he died of as pure black vomit as was ever seen.

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\* We are indebted to Dr. Fenner, of this city, for the following notes in relation to ship Rome, he having visited her in person:—"Ship Rome, Capt. Moulton—American vessel—arrived from Havre *direct* on 3d June, 1855—was hailed at quarantine June 2d, examined and passed—brought but little cargo and 162 German emigrants—had little sickness at sea, and this confined to children, seven of whom died. Most of the emigrants left for St. Louis June 4th. Capt. M. has now but one man on board—no sickness, and has heard of none in the neighborhood of his vessel. Ship Rome lies at post 24, about three squares above St. Mary's market."

June 17th—Herrman Ferringer—German—aged 20 years—last from Chicago—in New Orleans six months—entered ward 22—occupation, brewer—presented all the symptoms of “pernicious intermittent fever,” but died on the 19th with some symptoms of yellow fever—so decided as to lead to the prediction that black vomit would be found in the stomach after death. The autopsy revealed genuine black vomit in the stomach.

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#### HEALTH OF OUR CITY—JULY 25TH, 1855.

The 25th of July has arrived, and we find ourselves again called on to render an account of the doings of disease in our city during the past month. Our last report was anything but a flattering one, and we had hoped to be able, this time, to present our readers a more agreeable picture. To some extent these hopes have been realized, though there is nothing in our existing condition which can be construed into cause for self-gratulation. Our last report showed us to be just emerging from an epidemic of cholera, which had, in a few short weeks, killed near eight hundred of our citizens, in addition to a large mortality by other diseases. Since the issue of that report, the number of deaths by this disease have gradually declined, until it has almost disappeared—there being but 8 deaths reported for the week ending July 22d. In our last issue, however, we announced the appearance of yellow fever here by reports of ten undoubted cases which had been seen in the Charity Hospital. Since that time the disease has steadily increased, until for the week ending July 22d we notice 119 deaths from yellow fever alone. Up to the present time the total number of deaths by this disease reported is 201—to July 1st, 6; for week ending July 8th, 32; for week ending 15th, 44; and for week ending 22d, 119.

Concerning the origin of this disease for the present season we have treated elsewhere. So far as we are capable of determining, there appears to be nothing approaching a uniform type; we have noticed all grades, from that which is so mild as to be difficult of diagnosis, to that which partakes of the true congestive nature, and kills within forty-eight hours. So far, we are satisfied that fully nine-tenths of those attacked are recently (within nine months) from a cold climate; a large number are recent emigrants, and a number are foreigners who have been living in the Northern or Western States, who have recently come to the city, and among these we notice by far the greatest mortality in proportion to the number attacked. The cause of this we leave for others to determine, though we are inclined to attribute it to the irregular habits of the individuals—they, by their own confessions, proving the irresistible impulse of every Western young man to “get on a spree” as soon as he lands at our wharves, and an attack of yellow fever based on intoxication is about as little amenable to treatment as a patient in the collapse stage of cholera.

One very important point we have noticed in connection with the yellow fever in existence here, is that quite a number of persons have been attacked by the disease within three to six days after their arrival in the city. This certainly

tends to show that the existing fever cause, be it what it may, has acquired considerable potency.

It is not for us to speculate on the future; we have, as journalists, to deal only with facts; so far, however, we think we may safely say that we will be spared a re-enactment of the scenes of 1853. For the week ending July 23d, 1853, the number of deaths by yellow fever alone was 429; while for the corresponding week of this year we have but 119. This difference may be in *some* degree the result of a deficiency of material, but it cannot be altogether so; the fever producer must be less powerful than it was then.

We have noticed a fact this season, which, although nothing new, is certainly very interesting. Several persons who passed safely through the epidemics of 1853, 1854, and were then unacclimated, have had the disease severely within the past three weeks. One man is worthy of especial notice in this place. He acted as nurse in the Hospital during the summers of 1853-'54, and passed unscathed; and this season the poor fellow died of black vomit on the fifth day of the disease.

During the past two months we have had a considerable number of cases of typhoid fever. Although our physicians are by no means unacquainted with the disease, still it is something unusual to have it existing here to the extent it has latterly. We hear of its prevalence in various parts of the Southern country too, and the facts tend strongly to impress us with the belief that a further acquaintance with this disease will be forced on us: it really seems to have been gradually traveling towards us for several years past. Our annexed mortuary statistics show that not a few deaths have been caused by the disease in the past month. What the general plan of treatment has been we know not, though amongst our acquaintances the expectant mode seems to have the preference, and has been attended with success.

The reader will be somewhat surprised at the incompleteness of our mortuary record, but it is the very best to be procured from the office of the Board of Health—the only resource we have. We look with confidence to great and speedy improvement in this particular.

*List of Mortality in City of New Orleans for four weeks, ending July 22d, 1855.*—Total mortality 691, or a daily average of 24 $\frac{2}{3}$ —males, 245; females, 211; sex not stated, 235—under one month, 23; from one month to one year, 63; from one to five years, 97; from five to ten years, 26; from ten to twenty years 60; from twenty to thirty years, 169; from thirty to forty years, 96; from forty to fifty years, 56; from fifty to sixty years, 25; from sixty to seventy years, 22; from seventy to eighty years, 6; over eighty years, 5—yellow fever, 201—typhoid fever, 38—cholera, 66—still-born, 25.

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THE INDIANA JOURNAL OF MEDICINE AND SURGERY.—We are in receipt of the first number (July) of this Medical Journal, and give it a cordial welcome to our exchange list. It is issued monthly at Madison, Indiana, and edited by Drs. J. Jackson and T. W. Forshee. We wish the gentlemen the fullest success in their undertaking.



A HOMEOPATH DISGUSTED.—Carondelet street, the centre of the Hanemanic fraternity in this city, has lately been put in mourning by the apostacy of a German disciple of the illustrious father of infinitismals. This individual has abandoned New Orleans and globules, and betaken himself to Carrollton and mammoth doses. We saw a prescription of his not long since which shows how prone mankind is to run into extremes; it consisted of quinine, grs. LXLVI., and tartar emetic, grs. XVI., in eight powders, to be taken three times daily. The prescriber of such heroic doses of the so-called allopathic medicines, must be considerably disgusted with the remedial efficacy of the 30th dilution.

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“AN OUTLINE OF MEDICAL CHEMISTRY, FOR THE USE OF STUDENTS—By Howard Rand, A. M., M. D.”—This little volume purports to be a hand-book for the use of Students of Medicine, and as such it appears quite well adapted. For the copy before us we are indebted to the publishers, Messrs. Lindsay & Blakiston, of Philadelphia, through the courtesy of Thos. L. White, bookseller in this city.

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BOOKS AND PAMPHLETS RECEIVED.—Through the politeness of Mr. J. B. Steel of this city, we are indebted to the publishers, Messrs. Blanchard & Lea, of Philadelphia, for copies of “Tyler Smith on Leucorrhœa,” and “Gross on Diseases of Urinary Organs,” of each of which we will say more hereafter. We are, also, in receipt of the “Annual Announcements and Catalogues of the Medical College, of the State of South Carolina,” and the “Medical Department of the University of Nashville.”

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#### CHARITY HOSPITAL.

As is usual at this season of the year when yellow fever is prevalent, there is but little surgery in the Hospital. There have been several operations, among which, may be mentioned two amputations of the leg. The first, for caries of the tibio-tarsal articulation, died within twenty-four hours of the operation, having become exhausted by long standing disease, hectic fever and supuration; the second was rendered necessary by caries of the same bones, from compound commuted fracture, badly treated; amputation was performed below the knee, by the posterior flap operation. union by first intention took place, and the patient, now fourteen days after the operation, is on his crutches in the ward.

One operation for caries of the lower jaw is doing well.

In ward No. 2, a patient entered, who had received a charge of buck shot in the arm; about two inches of the ulna had been destroyed; the arm was otherwise injured; the circulation in the radial artery being uninterrupted, and the patient's general health good, attempts were made to save the limb; during his treatment, the ulnar artery gave way from sloughing, and was tied; he died seven days after his entrance into the Hospital, from purulent infection.

In the eye wards, two operations have been performed, one for cataract, the other for the formation of artificial pupil.

The cataract was uncomplicated soft lenticular, the operation by *broiement* was

performed; absorption is now going on, and the patient doing well. The operation for artificial pupil did not succeed. The patient was placed under the influence of chloroform, a puncture made in the cornea, and the fine hook introduced for the purpose of detaching the iris. This was, however, not accomplished, partly because, the hook was imperfect, but more especially for the reason, that the patient recovered his sensibility before the completion of the operation, and consequently, frustrated all attempts to retain the eye in a fixed position. No bad result has followed the operation from inflammation, and there is nothing to prevent success at some future time.

Three cases of peritonitis have been successfully treated by the administration of mercury internally, together with frictions of mercurial ointment and extract of belladonna, applied to the abdomen.

Since the first case of yellow fever on the 30th of April, there have been, up to to-day (27th July,) by this disease, 199 deaths, and 89 discharges. From the 1st of July, the deaths have been daily from this disease, as follows:

July 1st, 3; 2nd, 2; 3d, 1; 4th, 3; 5th, 4; 6th, 3; 7th, 1; 8th, 3; 9th, 2; 10th, 1; 11th, 8; 12th, 6; 13th, 8; 14th, 6; 15th, 4; 16th, 9; 17th, 16; 18th, 8; 19th, 11; 20th, 15; 21st, 12; 22nd, 11; 23d, 23; 24th, 14; 25th, 11; 26th, 12. The report for the 27th is not complete.

The number of admissions of yellow fever do not seem to increase much, and the disease is of a type much more mild, than that of the summers of 1853 and 1854.

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CHARITY HOSPITAL REPORT—From July 1st to July 28th, at 9 o'clock, A. M.

Admitted,	-	-	-	-	-	-	-	-	-	1,190
Discharged,	-	-	-	-	-	-	-	-	-	815
Died,	-	-	-	-	-	-	-	-	-	286
Remaining,	-	-	-	-	-	-	-	-	-	617
Births,—Males, 7; females 5.										

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## Excerpta.

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SECONDARY SYPHILIS TRANSMITTED BY VACCINATION.—The *Scalpel*, a medical journal of Brussels, reports a circumstance, which we should not have reproduced, did it not tend to give countenance to an unfortunate prejudice, popular even among those best informed. We subjoin it with some reflections of our own.

“*Bamberg, December 20th, 1854.*—A singular case of transmission of secondary syphilis by vaccine matter, has lately occupied the attention of the tribunal of Bamberg.

A physician of Upper Franconia, having taken vaccine matter from the arm of a child, affected with a papular eruption of the face and body, vaccinated a number of children with it, eight of whom afterwards presented different forms of secondary syphilis.

From the evidence of witnesses, it was shown that after the vaccination, no well developed pustules were formed, but, that small vesicles containing pus and surrounded by others smaller, made their appearance, and were ruptured before the usual time, leaving an ulcerating surface. These ulcerations were difficult to heal, in some instances they did not heal. Nine grown persons who had charge of these children, were affected by the virus. And it was not until large abscesses, pustules, and condylomata had been observed both in the adults and children, that the affection was suspected to be the result of the vaccine taken from the diseased child. The nature of the disease was finally discovered, but too late.

Among the adults, she who suffered the most, was the mother of the child affected with condylomata of the anus and genitals. She had been in the habit of carrying it on her naked arm, which, became ulcerated by constant contact with its secretions, and gave origin to general syphilitic affection. To make the misfortune greater, the woman became enceinte before she was cured, and gave birth to a syphilitic child.

It is to be particularly noticed, that several children inhabiting a different locality, who had been vaccinated with this same virus, had normal vaccine pustules, and were in no wise injuriously affected. An examination of the mother of the child, cause of the evil, showed that she was cured, and bore no signs of disease of the genital organs—but she had had syphilis before conception, but at that time, appeared to be well. (*This case shakes the doctrines of M. Ricord, who maintains that chancre alone is inoculable, and that chancre alone is contagious.*)

The physician who performed these vaccinations, was condemned to one year's imprisonment, for having failed to make use of the requisite degree of prudence in the exercise of his art. His penalty was finally reduced to six weeks imprisonment."

The above observation if admitted as a fact, would be calculated to strengthen an injurious prejudice which now exists, which is, that vaccine matter taken from a diseased child, is susceptible of transmitting to the individual vaccinated, the disease with which the subject who furnished it was affected. The author of the article in question, pretends also, by the observation, to prove the truth of another opinion, which we believe to be erroneous, that is, the possibility of transmitting secondary symptoms of syphilis from individual to individual.

Let us examine these two assertions successively.

Can vaccine matter, taken from an unhealthy child, transmit to others upon whom it may be used, the disease of the individual who furnished it? NO.

All authors who have thoroughly investigated the subject of vaccine and vaccination, agree upon the fact, that vaccination is always identical, that it cannot *blend or ally itself with another virus*, or be modified by it; and that the disease is developed, and goes through the different phases of its evolution, in a manner entirely independent of any disease with which the subject vaccinated may be affected. To support our assertion, we will not go back to the numerous experiments which have been made demonstrating this truth, but content ourselves with citing three or four of the principal authorities.

Guersant and Blache express themselves as follows: When inoculation is made with a mixture of the vaccine and small pox virus, but one of these diseases is produced; or if both should be developed together, their course is entirely separate and distinct, and each has its own peculiar characteristics. In a great number of experiments made by the committee on vaccination, (Rapp. du Comité de vaccine, p. 401, & 413,) or by its correspondents, vaccine virus was taken from pustules, which had been purposely produced in the midst of a variety of skin diseases, such as favus, scabies, scrofulous ulcers, etc., and in every case, nothing but the vaccine pustule could be reproduced.

Dr. Taupin, late *interne* at the *Hôpital des Enfants*, vaccinated in the course of four years, more than four thousand children, in every variety of disease and health of both sexes. He noted with care the result of these vaccinations, was particularly careful to observe what modifications might be produced in the virus by different diseases, and what influence these could have upon other individuals upon whom the matter was used; and he concluded that vaccine virus taken from children affected with chronic or acute diseases of whatever character, was as

active as that from the purest source; that the vaccination was as effective and complete; and finally, that vaccine virus can transmit no disease, either chronic or acute, contagious or non-contagious. Vaccine taken from subjects affected with rachitis, scrofula, syphilis, tubercules, chronic eruptions of the scalp, etc., never communicated anything but vaccine.

Far be it from us, adds MM. Guersant and Blache, to conclude from this that it is a matter of indifference whether vaccine virus from a healthy or an unhealthy person be used, but we report these facts, well established as they are, to do away with that prejudice which attributes to unhealthy vaccine, diseases which may sometimes supervene upon vaccination, even long after it has been performed.

Finally we will cite M. Bosquet: he says, it has frequently happened, either from ignorance or design, that vaccine has been used, taken from children affected with syphilis. What has been the result? The vaccine has always been reproduced in all its purity without causing any unfortunate effects, and without ever giving rise to a suspicion of its source.

It may be objected that we only cite authorities, and that one *fact* is worth all of them. We will answer that in medicine those alone are authorities, who being competent men have established their opinions on well observed facts. Was the case in question correctly noted? We say no, that it is incomplete and badly reported, and we will prove it.

Several children are vaccinated with vaccine virus taken from a syphilitic subject. If the vaccine had been syphilitic it ought to have infected all those upon whom it was used—but far from it. A part only of those vaccinated are affected, (in what proportion we are not told) and several children in another locality upon whom the same virus is used have healthy scabs, and are affected in no wise prejudiciously. Is not this circumstance sufficient in itself to demonstrate once more the absurdity in medicine of the axiom, *Post hoc ergo propter hoc*? If a child break its leg or is seized with pneumonia after vaccination, will any one attribute the circumstances to the vaccination? In our case the vaccine did not transmit constitutional syphilis, but it did not prevent the development of this syphilis in children affected hereditarily. In fact, nothing has been shown to prove that the eight children did not *inherit* the disease, the symptoms of which were observed. Was a rigid inquiry made into the previous history of the children or their parents? Had they presented before the vaccination any symptoms of hereditary constitutional syphilis, such as spots, pimples or ulceration? Were the parents of these children examined, or their history obtained? Is it certain that none of the parents of these children were affected with syphilis, as was the mother of the child from whom it is pretended the rest were infected? The reported observation being silent on these points, we are authorized to conclude that the necessary investigations were not made, and without this the case is completely without value.

We are not informed as to whether the parenthesis in italics, which terminates the penultimate paragraph of the observation, is due to the German journal or the Belgian, which has copied it, but wherever it may come from we think it can be shown that it no wise militates against the doctrine of M. Ricord. With the part of the observation relating to the children, we have done. But something remains quite as singular.

The mother of one of the children carries it in her naked arms, upon which there soon appear ulcerations in different places where contact had existed with the secretions of the child. This woman is examined and condylomata of the anus and vulva are discovered—but the reporter of the case does not tell us a word as to whether the ulcerations preceded the condylomata or *vice versa*. It seems that no investigation was made for the purpose of discovering whether she had presented previously signs of constitutional syphilis, or whether or not her husband was healthy. There is, moreover, no mention made of the antecedents of the nine persons who took care of the children, and who at the time of examination were found to be infected. If the mother of the child from whom the original vaccine was taken was proved to be syphilitic, what is there to prevent the admission of the existence of the disease in the other nine? Shall science be constituted of such observations as this?



We will conclude by saying that this case, cited to prove the syphilitic nature of vaccine matter taken from a syphilitic child, is not scientifically reported, and is only fit to serve as a text for the plea of incompetent advocates, to hide the libertinage of parents and satisfy a dangerous and ridiculous prejudice.—*Gazette des Hopitaux.*

REMOVAL OF METALS FROM THE SYSTEM BY GALVANISM. *By G. Huff, M. D., Lexington, Kentucky.*

*To the Editors of the New York Medical Times:*

GENTLEMEN—Having experienced the beneficial effects of galvanism in extracting metallic poisons from the human organism, and believing it to be a subject of much interest to the profession, I now place at your disposal a report of my experiments.

Very respectfully yours,

G. HUFF.

CASE I.—Mrs. W—, aged 27 years, of lymphatic temperament, with auburn hair and white skin, had been under treatment for diseased spine fifteen months. During this time she had taken very large quantities of mercury, which, her subsequent medical attendant stated, produced paralysis of the lower extremities.

I was called in consultation by the advice of her physicians, and it was decided that she should be put under treatment by galvanism. Her physicians having thrown upon me the entire responsibility of the case, I took charge of her; and one day while making an application of this potent agent to the spine, the feet having been placed in a metallic bathing-tub with acidulated water, her husband suddenly called my attention, exclaiming at the same time, "See the mercury!" On making an examination, I found several globules of metallic mercury lying on the bottom of the tub. I continued this (electric) treatment for a long time and she ultimately recovered, and now enjoys the powers of locomotion most perfectly.

CASE II.—Mr. B—, aged 40 years, of nervous temperament, with dark hair, white and thin skin, had been treated for syphilis for a long period, and had been repeatedly salivated, from which he had suffered severely in the joints. The capsular ligament was so much elongated as to cause luxation of the head of the femur; separation of the carpus of each hand had taken place, and the metacarpal joints of the fingers were very much enlarged. He had been under treatment by physicians who stood deservedly high in the profession, and had visited warm springs in Arkansas by their advice. At this time he could scarcely move with crutches, even with the help of two attendants which he took with him. He remained there one winter, and returned without having obtained any relief. His friends then advised him to apply to me, and, with the consent of his physicians, he did so. On examination of his case I concluded to treat him, and commenced with warm baths, which invariably left him worse, the joints becoming more stiff and painful, and with less mobility. Believing that the remote cause of this aggravation of his disease was the presence of mercury in his system, I was induced to attempt to extract it. To accomplish this, after having placed him in a porcelain bathing-foot tub, with acidulated water, and a metallic plate beneath his feet, I completed the circuit, and after the lapse of twenty minutes I discovered a light-white precipitate, and the impress of his toes left on the plate of a light bluish color, with silvery lustre. I repeated this operation several times, and then commenced the galvanic treatment for rheumatism, and infused iodine into the joints in order to produce absorption of abnormal secretion that had formed there. From this time he commenced to improve, and went on improving without a relapse. All the joints have now recovered their normal condition, with the exception of the left hip-joint, the femur of that side now remaining seven-eighths of an inch below the right, although it has ascended three-eighths of an inch during my treatment; his general health has been very much improved; in fact, he says it is now as good as it has been at any period of his life.

CASE III.—Mrs. N—, aged 28 years, of bilious temperament, small size, hair and eyes black, of a very high order of intellect. At the birth of her second child there was very profuse hemorrhage, and much inflammation was superadded in consequence of medicines having been imprudently given by her physicians to facilitate labor. For the purpose of suppressing the hemorrhage and restoring

the uterus to a healthy condition, sugar of lead was given in small doses, and its use continued a long time. This treatment resulted in *lead palsy* (the total loss of muscular contraction of the lower extremities). In order to extract the lead from her system, I commenced the treatment by galvanism in the same manner as in the foregoing case, and with the same results, except that the precipitate was of a dark gray color, and the impress of the toes left on the plate was of a darker hue. When the paralysis was nearly removed, partial amaurosis set in, and ultimately became total. I treated this without benefit, although I think the treatment has not been fully tested, as she was obliged to return home, in consequence of domestic cares, sooner than I anticipated.

LEXINGTON, Ky., May 8th, 1855.

[The preceding communication contains results which will doubtless be novel to most of our readers, and which would seem to promise to be of value in a therapeutic point of view. It derives additional interest from the fact that, at the meeting of the Imperial Academy of Medicine of Paris, held January 29, 1855, a note was presented from MM. Verguès and A. Poey, on the new application of electro-chemistry to the removal of metals from the system. We extract the following account of the disease and process from the *L'Abeille Médicale*, 15th Feb., 1855:—

M. Verguès, who had on the back of his hand an ill-conditioned ulcer, caused by the introduction of metallic substances in the process of gilding and silvering by galvanism, on plunging his hands into an electro-chemical bath at the positive pole, found, to his great surprise, a metallic plate in contact with the negative pole covered, at the end of a quarter of an hour, with a thin layer of gold and silver. A few baths proved sufficient to radically cure the ulcers, which had previously resisted the most active means. The first experiment was made at New York, the 16th of April, 1852, and was followed by several others, which had led to the introduction of a new therapeutic mode of removing metals from the system.

The patient is placed up to the neck in a metallic bathing tub, isolated from the ground, and made to rest in a horizontal position upon a wooden bench, the whole length of the body, which is to be also isolated from the bathing tub. The water is to be acidulated with the nitric or hydrochloric acid for the removal of mercury, gold, and silver, and with sulphuric acid for the removal of lead.

One extremity of the bath is put in contact with the negative pole of the pile by means of a screw, and the patient takes hold of the positive pole sometimes with the right hand and sometimes with the left. The arm is held up by supports in contact with the seat. The extremity of the positive conductor which the patient holds is armed with a massive iron handle, wrapped around with linen, to diminish the calorific action of the current, which is very powerful, and which, without this prevention, would burn the hands.

The patient being thus placed, the positive current enters either by the right or left arm, circulates from the head to the feet, and is neutralized at the negative pole on the sides of the bathing-tub. Being isolated, from direct contact with the negative pole, as well as from the ground, the electric fluid radiates from the body into the bath, forming a multitude of currents from the entire surface of the body, which, after having traversed the internal organs and even the bones, neutralize themselves upon the negative side of the bathing tub.

They say that they have thus withdrawn from the femur and tibia of a patient a large quantity of mercury, which, according to the opinion of several physicians, had remained there fifteen years.

The paper was referred to a commission, consisting of MM. Dumas, Rayet, and Cl. Bernard.

The *Virginia Med. and Surgical Journal*, for May 1, 1855, contains also (in addition to the above) an account of an experiment made before the members of the Faculty of Medicine of Havana, in which a similar result followed the use of the same agent.

The metallic spots formed by this process are said to vary in size from that of the head of a pin to the size of a pea, while some are microscopic.

In a letter received from Dr. Huff, subsequently to this communication, he says that he had never seen the process, nor read in any work respecting what he calls "his method of extracting metals from the human system." He says, "my mode

is constantly demonstrated by the ordinary course pursued for the electrolysis of metallic salts, by those engaged in electrotyping and electroplating." He speaks also of the solution of urinary deposits in the bladder by galvanism without any *difficulty or pain*, and promises to communicate some interesting results on the subject.

The report to the Imperial Academy of Medicine has been copied in different journals in our country, and has been hailed as embodying a valuable contribution to therapeutics; and if future results confirm the hopes thus entertained, the paper of Dr. Huff will be invested with additional interest.—*Eds. N. Y. Med. Times.*]

MAYOR CONRAD'S ADDRESS TO THE AMERICAN MEDICAL ASSOCIATION.—*Mr. Chairman of the Committee of Arrangements:* I thank you, in the name of the community which I have the honor to represent, for your eloquent introduction of our friends to the authorities of the city, and to this the Hall of Independence.

*Gentlemen of the American Medical Association:* I am proud of the privilege of extending to you, in the name of the government and of the people of Philadelphia, a most cordial welcome.

I bid you welcome to our city—a city which, deriving a cherished distinction from the profession which you adorn, is eager, now and ever, to requite it in her tribute of respect for its professors. I welcome you to our people, whose intercourse for many a year, with you or your brethren, has inspired a feeling which, reserved as we are sometimes said to be, will, I doubt not, burst into earnest and unambiguous expression, before you leave us.

I welcome you, gentlemen, to this Hall, but not as strangers or the sons of strangers—for it is your own. As the temple and territory of Delphos, in the wildest domestic perturbations of Greece, afforded one sacred area over which the cloud of discord never gathered, one altar whose worship was never invaded, this spot, consecrated to our common American glory, knows no lines of latitude, and belongs, in truth, no more to us, whose peculiar privilege it is to inherit its guardianship, than to our brothers—to you. In coming hither, therefore, you come home. These precincts have been hallowed, for all time, by the heroic virtues of your and our fathers. This is the fountain from which the living waters of American liberty were first drawn, and it is therefore most sacred—(wo to the generation in which it ceases to be sacred!)—but, like the well of the patriarch, all the tribes of Liberty's Israel own here an equal right, and owe here an equal homage.

In no sense, then, can I greet you as strangers—for yours are names familiar to every American proud of the science of his country; and those who are united, by this Association, in a cause so lofty as that eloquently characterized by your Chairman, may not only claim the universal and acknowledged privileges of the Republic of minds, but the rights of a nearer and dearer charter, the Brotherhood of beneficence—the kindred claims of noble hearts, knit in the highest and holiest of human aspirations. In this spirit, with the most fervent and fraternal sentiments of respect and regard, I greet and welcome you.

You are right, Mr. Chairman, in claiming, amid the associations which hallow these precincts, a peculiar privilege for your profession—a profession which not only sprinkled, with the earliest sacrificial blood of the Revolution, the highest altar upon which Valor vowed and dedicated our country to freedom—I refer as you have referred to Dr. Warren and Bunker Hill—but which, in every struggle for the enlargement and enlightenment of human destinies, has been eminently distinguished for courage, zeal, and fidelity to the rights of man. You have, therefore, a peculiar right to claim kindred here, and have that claim allowed; and within these walls, which witnessed the zeal of Rush, it would be a treason to virtue to forget, that one of the lights of your profession shed glory upon the solemn debates of this hall, and was foremost amongst those that bade yonder bell,\* (preserved and devoted to the veneration of posterity), with its iron tongue,

\* *The Liberty Bell.*—This is the bell which was rejoicingly rung, from the steeple of the old State House, when the Declaration of Independence was originally read, in July, 1776, to the thousands assembled in the State House yard, now Independence Square. Upon this bell—cast long before the Revolution, and brought from England in the colony times—are the prophetic words of Scripture quoted—"Proclaim liberty throughout all the land, to all the inhabitants thereof."



to PROCLAIM LIBERTY THROUGHOUT ALL THE LAND, TO ALL THE INHABITANTS THEREOF.

It is the glorious peculiarity of your profession that, while Ambition, in its ordinary and most applauded paths, plays the part of the *Destroyer* and wins glory at the expense of human life and happiness, you and yours, with a more exalted civilization, a nobler heroism, have ever *sought to save*. Next to the highest of all human courage—if, indeed it be merely *human*—that of the martyrs of religious Truth—the courage of the physician, whether on the battle-field or in the lazaretto, the courage of science and humanity, is the most sublime, and the best entitled to the *clarum et venerabile nomen*. The vulgar courage of the warrior, under the base stimulus of passion, or the low greed of applause, can hardly be compared to the noble intrepidity of the surgeon, who gleans in the ruthless and red-handed reaper's path, the leavings of the battle; and still less with the hero of the hospital, who encounters the grim antagonist in the horrid silence and gloom of the pestilence. Imagination can hardly embody an instance of human courage and virtue more sublime and unearthly than that of the physician, who, in the midnight of a plague-stricken city, treads the fetid solitudes of its alleys, and, entering the devoted hovel of the wretched, ministers—while only Pestilence and Misery, Death and God look on—to the perishing. I need not step from this spot to grasp the hand of many a hero who claims no laurel—many a noble philanthropist whose sacred labors, in scenes like these, have been unmarked, save by the Eye that never slumbers, and remembered only by Him who alone can reward.

To such a profession, one venerable from its antiquity, noble from the grandeur of its objects, illustrious from its achievements, and which demands every aid and energy of genius and science, of head and heart, that dignifies the race, it is not strange that go where it may, a ready homage greets, and a ready blessing attends it. In our own city, all that is noble in patriotism, all that is exalted in science, all that is bright and beautiful in the arts which refine society, all that is lovely and cherished and holy in private life, combine to render the profession sacred and dear to us.

There are few living, to whom some one death in the past is not the sole event and solitary memory of the survivor's life—to him a lonely pyramid in the melancholy desert; and to such a mind and memory, the *debt of the death-bed*, where science, rendered holy by its office, ministered, though never paid, is never repudiated. I never knew a good man, still less a good woman, who had not such a debt—a debt which bankrupt gratitude cherished with its holiest affections, and sanctified with its devotest memories.

In these times, when the omnipotence of associated effort is invoked for so much that is of dubious merit, it is a gratifying spectacle to behold the enlightened professors of the most exalted of all arts—men sage and grave, unselfish and unassuming—forsaking the home to which they are bound by the affections and afflictions of thousands, by wealth, fame, and influence, to wander, wearily, away upon a pilgrimage of hundreds of leagues, in the cause and interests of the human family, its security, health and happiness. For more than ten years, the representatives of your profession have thus gathered in Convention. What other body of our citizens have made a like effort—a like sacrifice? Selected from the most eminent of the profession, the delegates have been men whose years, like their virtues, were many. How difficult must have been, to them, the effort to burst through the bonds of a relying and clinging practice! How great the labor and how heavy the sacrifice! They have already visited, in this duty, the cities of every section of our wide country. How many have fallen by the wayside? How many martyrs could you not number in this cause? How many of the good and great of the profession have, in the benevolent pilgrimages, joined the ranks of the thousands who have sacrificed themselves, at the requisitions of duty, as recognized and enforced by your self-imposed laws—joining the dead in the effort to aid the living? The epitaph of the Spartans at Thermopylae might well commemorate the virtues and the fate of these martyrs. But if the cost has been great, the results have been commensurate.

Of the professional advantages attained, though I know them to be invaluable, I



will not presume to speak ; but I may be permitted to state, as health is the most important subject of municipal provision and care, that the Transactions of the Association, which I have examined with great interest, comprise much that merits the attention and will reward the respectful consideration, of the municipal governments of the Union.

It is natural that Philadelphia should feel, as she does feel, a profound interest in the cause of medical education in this country. She cannot, of course, forget that it was here that the first medical college was established in this country ; that its merits and success extorted a reluctant transatlantic tribute of admiration ; and that, progressing rapidly, but wisely, it achieved and maintained an equality with the most celebrated institutions of the Old World. As the cause of Medical education has expanded and institutions worthy of the cause and country have sprung up, each triumph, thus attained, has been regarded here as the successful outbursting of an offshoot from the primary effort ; and Philadelphia, while rejoicing in the expansion and elevation of medical education throughout the land, has almost fancied—so earnest is her interest in medical education—that she had a right to indulge a *parental* pride in all that advances that interest.

These genial feelings have been maintained, in all their fervid and early freshness, by constant intercourse with all sections of our country. The ingenious and gallant youths that have come hither for medical instruction have, in their unstudied intercourse, exhibited the character of their respective States in a light so generous and exalted, as to win our affections, not only for themselves, but for the communities and States which could exult in them as their own. Winter after winter, we have had many hundreds of these noble young spirits among us. And let me remark that, rigorous as I am said to be in the administration of the law, I have yet to know the first occasion to rebuke, much less to punish, a medical student. We have found them as gentle and decorous in their deportment, as they are exalted in their aspirations ; and had Philadelphia, eminently catholic in her affection for her sister communities—needed a lesson of love and loyalty, these high-hearted missionaries would have taught it. This interchange of sympathies has endured for the third of a century—may it last forever ! The youths—youths no longer—who formerly bore those sentiments to the remote sections of our republic, stand before me now as the revered sages and ornaments of their profession, meeting here the evidences of a reputation which had preceded them. and has long been cherished by us. And who can tell what have been the results of this kindly interchange of kindly feelings ? It has doubtless been felt in every commercial, social, and political relation of life, correcting the prejudices, harmonizing the discords, and subduing the dangers of our common country.

We realize these facts. We recognize, in the members of an enlightened profession like yours, so many patriots and philanthropists, engaged in the great and general interests of the human race ; and, apart from the mere scientific acquisitions of your annual meetings, we perceive in them results auspicious to all that we cherish, all that is kindly, forbearing and conservative, between man and man, party and party, State and State, section and section ; and, so regarding them, we hail and greet you with a welcome as sincere and cordial as the heart can conceive, or the tongue can utter.

RE-VACCINATION IN THE RUSSIAN ARMY IN 1853. — Vaccination was performed upon 44,652 men in the Russian army during the year 1853 ; and of the whole number re-vaccinated, 28,329 had the regular vaccine eruption — 5,933 had an irregular eruption, and 7,664 had none at all, making a proportion of sixty-nine in the hundred of those previously vaccinated. Of the whole, 44,652 men, 32,642 presented clear evidences of the vaccine cicatrix ; in 7,643 the character of the scars was doubtful, and 4,367 had no scars at all.

During the twenty years ending in 1853, the proportion of successful re-vaccinations has almost constantly increased, the first and second years of this series (1833-'34,) not giving quite half the number of successful re-vaccinations in the hundred as the two latter, (1852-'53.)

During the year 1853, 106 cases of varioloid, 25 cases of chicken-pox, and 7 of small-pox, occurred in the army.

**SMALL-POX IN UTERO.** — *To the Editor of the Lancet.* Sir: In October 1853, I attended several children of one family, for small-pox. An elder daughter, who was in the eighth month of her pregnancy, attended upon them. She was seized with small-pox, although previously vaccinated, as evidenced by the scar on her arms; she passed through the attack without any untoward symptom. About fourteen days after the desication of the pustules, she gave birth to a fine son. The child, on the eighth day, was attacked with sickness: on the tenth day, small-pox of a most virulent kind broke out: on the fourth day, from the first appearance of the eruption, the child died of convulsions. The above case proves the fact, that the mother may have small-pox, yet the child in utero escape. I am inclined to think, in the case reported by Mr. Osborne, in the *Lancet* of the 19th inst., that the child had not had small-pox: hence, the success of the vaccination. This subject is one of great interest, and worthy of careful investigation.

I am, Sir, yours, &c.

T. H. WARDLEWORTH, M. R. C. S., Eng., &c.

Stockport, May, 1855.

**RECENT CHEMICAL DISCOVERIES.** — A recent chemical discovery proves that alcohol may be made synthetically from coal-gas (and probably other carbo-hydrogens and water. If the discovery prove remunerative, it may very much modify the expense of quinine prepared by alcohol, tinctures, and ethers of all kinds, and would, we are sure, be a welcome boon to curators of museums and others. The eminent pharmaceutical chemist of Dublin, Donovan, lately took advantage of the ice and snow, so plentiful in that city, to examine into the properties of some pharmaceutical ethers, which would be impossible without large masses of ice and snow: amongst the rest *chloric ether*, now so frequently found in prescriptions. This able chemist states, that there is no such compound at all as chloric ether, and what is known as perchloric ether, detonates violently by even shaking the bottle. Surgeons prescribe chloric ether, but the patient gets Dutch oil, more generally, chloroform in an insoluble state, or spirits of wine: but the nearest approach to chloric ether is muriatic ether, which he succeeded in obtaining, or perchloric ether, which he gives a caution not to use, as it explodes. — *London Lancet.*

**THE MEDICAL FORCE IN THE EAST.** — From a return made by Dr. Andrew Smith, it appears that the total number of practitioners sent to Turkey, including civil surgeons, apothecaries, dispensers, and dressers, is 529. Three or four civilians are on duty with the army, engaged under the sanction of Lord Raglan.

The total numbers of medical men who have died or been invalided: Died, 29; Invalided, 38, of which number, 7 have rejoined.

The total number, including apothecaries, dispensers, and dressers, at present with the army, up to the latest date, is 469. From another return, we discover that of this number, 297 are under thirty years of age, and 191 are under twenty-five years. — *London Lancet.*

**STATISTICS OF THE MEDICAL PROFESSION.** — Professor Escherich, of Wurzburg, has just published tables which would establish that the mortality is greater amongst medical men than in other professions. The author takes an aggregate of 15,739 persons belonging to the following classes: Medical practitioners, Protestant and Catholic clergymen, professors, schoolmasters, the bar, and those employed by the Board of Woods and Forests. He finds that three-fourths of medical men die before the age of fifty, and ten-elevenths before sixty. Out of 1,168 medical men alive in 1852, four only (8.34 per cent) were more than eighty years old. The ten oldest practitioners numbered together 792 years, whilst the ten oldest men taken from any of the above-stated professions, presented much higher figures. Catholic clergymen numbered 872 years, professors and schoolmasters 875, and Protestant clergymen 865. Out of 100 medical men, only twenty-six had reached fifty years: and it is well known that, according to Casper's statistics, the proportion is less favorable in Prussia, as only nineteen per cent. reach the age of fifty. — *London Lancet.*

NEW ORLEANS

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Anniversary Address to the Medical Society

OF THE STATE OF LOUISIANA — FEBRUARY, 1855.

By E. H. BARTON, A. M., M. D., *President.*

*Gentlemen*—I present you my cordial greetings at this Anniversary Meeting of the Society. I welcome the return of this day, because it is one devoted to the intercourse of genial minds in the sacred cause of humanity; and I invoke it, because in the small but noble band I see around me, I recognize those who have consecrated their lives, amid years of toil and much endurance of no ordinary privation, to the amelioration of the physical sufferings of our fellow citizens. I need not say to you how erroneous the public sentiment which characterizes the practice of medicine as not only pleasant in pursuit but productive of affluence in the result. No statement is fraught with less truth or more mischief. Its pleasures arise from the consciousness of doing good—from the cultivation of science and the development of great truths, that must, when applied, benefit our fellow men; thus received, it is a great philanthropic calling, appointed by Providence as the means of ameliorating the condition of our kind.

The opinion is productive of *evil*, because its tendency is to invite into the sacred temple consecrated to Medicine, many whose sole object is a love of gain—a mere mode of living. I need not say where this is the case, the noble incentive—the great principle which gives the impulse to the cultivation of science—the almost divine feeling which constitutes the ambition to excel in it, a high virtue—no longer dwells in the heart. It is this which makes the distinction between the exercise of the profession as an art, and its cultivation as a science; the first keeps it on a level with the trades—degrades its high calling—paralyzes its lofty impulses, and



gives birth to the petty rivalries and vulgar animosities of low bred minds ; while the other elevates the mind to the level of that high and ennobling science, which was once thought worthy of deification, and which has ever since dignified the intellect, and occupies the earnest study and attention of some of the best of our race.

It is to be regretted that this feeling is not a universal one, pervading the faculty of the entire State. We have a noble example set us in our adjoining sister States, when, as the elder, it should have been set by ourselves. But, be it as it may, it is not to be permitted to us in this enlightened age of progress, to stand still while all the world is advancing in knowledge. The cause of tardiness in participating in these honorable labors is a mystery to me. Every invitation has been extended to our brethren in the rural districts to unite with us ; every courtesy felt and offered ; the honors of office have been participated in with them, and opportunities of distinction on committees ; all in vain ; and with the single exception (*promised*) which this year presents, and with the application for connection with us, as the parent Society, by the Medical Society of Natchitoches, little or no countenance has been shown to the State Society, although the honor of suggesting it originated in the country. Surely, this feeling can arise from no unworthy spirit of rivalry. In politics this may exist, for the motives there are impure—it is tainted with the thirst for power and love of gain ; in pure science these have no pabula for their support ; here, besides the abstract enjoyment in its cultivation, the only legitimate competition is the spirit which propels to do the greatest good to the greatest number. There is no doubt but that our Society would have succeeded much better if we had, from the first, published an independent annual volume of Transactions. Many, if not all, and I believe all the articles read before it, have been worthy of such an enduring record, and we should have been able now to look at the fruits of our labors with some pride. The desire to participate in such a record—the feeling that prompts to the wish to live in after times, cherished in the memories dear to us—the hope to be remembered, and our labors, or graves, pointed out, as of one having essayed to do, or accomplished, something for the good of our kind, is, I hope, a strong feeling with most of you to stimulate labor, and impel you on in an honorable career of usefulness. It is also a parental feeling. Such a memento, or monument, as may be, tends to connect us together and induces us to feel the necessity of continued exertion, to add to and perpetuate it ; and when I cast my memory back to



what most of these reports have been, and compare them with those of (what may be considered) our rival States, in this noble competition, there is left no cause to blush or be ashamed. It is never too late to repair the error we have committed.

The Report of the Board of Administration will show you the financial condition and the practical working of the Society during the year.

Gentlemen, my conscience tells me that I owe you some explanation for the omission of an act of duty to you last year, in the fulfillment of an appointment I had the honor to receive from your organ, (my predecessor) to report upon the Meteorology, the Vital Statistic and Hygiene of the State. It was physically impossible for me to fulfill that duty, on account of previous occupation and engagements to the public, in much the same line of research. Those labors are now before you, and I present them to you as some testimony of the honor I felt in the appointment; and my conscience acquits me of any intention of evading a sense of the duty I owed you under it—for, excuse me for saying, that it has been my boast through life, to perform every duty to the extent of my ability.

During the present year I could not prevail upon any of our colleagues to accept the post of chairman appertaining to that important committee. I trust you will concur with me in the opinion I entertain, that the information derivable or due from such a committee is too valuable and too much required, situated as our city is, to permit a hiatus to exist in our records upon the subject. With this belief I shall endeavor to present you such an outline, at least, of what the meteorology and vital statistics of the city has been, during that period, as my very limited time and opportunities from other urgent engagements has enabled me to prepare for you.

Gentlemen—Two disastrous years have afflicted our State, particularly our stricken city, with a double epidemic of yellow fever and cholera. In the dispensations of an all-wise Providence, His goodness permits no evils without allowing some compensating remedy to be within our reach. Our benevolent Creator acts upon his creatures through secondary means,—no effects result without adequate causes. The resultant duty is clear; it becomes the solemn obligation of society, and it is the more especial duty of that portion of it constituting the medical profession—as the organ and teacher of society in this particular calling, to which they are designed—to trace out these causes, to investigate the relations of cause and effect, so as to indicate the proper preventive or remedial measures. To correct er-

rors, to remedy defects, to remove causes and prevent their repetition, it is vitally important in CURATIVE, and how much more so in PREVENTIVE medicine, to understand them — to ascertain if this is casual — a mere coincidence — or, if it is constant, bearing the relation of cause and effect; thus becoming a law of causation, it is necessary that all the agencies appertaining to it should be thoroughly understood. It is not always *indispensable* in *curative* medicine that the causes of every malady should be made known previous to treatment; it constitutes the basis of all knowledge in *preventive* medicine. In confining our views to curative medicine, the profession will be narrowed down to most contracted limits, the greatest benefit expected by society from our labors will have been lost, and the profession looked upon not as a body of scientific men, but as traders upon a small capital — tinkering the ills of the public as much for our own pockets, as for the welfare of our patients; thus making the pecuniary interest of the physician to thrive just in proportion as the public suffers. It is pretty extensively believed that where the profession is most extensively employed, its grade and standing is correspondingly high in public estimation. This is not so, as the most cursory glance at the condition and standing of the profession in different countries will abundantly show. The social and reputable position of the faculty depends more in refined and elevated society, upon the scientific standing of its members and their cultivation of the higher branches of the profession, than the successful accumulation of wealth. Empirical medicine is more productive in a pecuniary point of view than scientific medicine; yet, it never receives the respect of society. It is selfish in its beginning and in its end. It never does or pretends to do any thing for the great public; social life, public respect, the noble and patriotic impulse of benefitting the country never influences them; the great sacrifices that make ambition virtue, is never made by them, and their success in any community is a pretty fair criterion of its relative standing in intelligence. Science has no connection with mercenary motives. In proportion as the profession is pursued with this end, its original object is degraded; it allies itself with the money God, and clothes itself with empirical pretensions. True science is always diffident; as we advance in knowledge our own ignorance is better known to us; the mist drops from the mental vision; the boundless ocean lies open before us; the horizon extends as elevation is reached; telescopic vision is increased, until all tends and all ends in the bosom of the giver of all light and all intelligence.

But, take a nobler view of it — and it is the special advantage and true interest of our calling to take this view — as it is known best to prosper as the community does in which it is situated. There is a large class of maladies known as *preventible*; they consist of that class which constitute in all sickly countries the main outlets of human life; to fevers alone is imputed one-third to two-thirds of the mortality, in the estimate of eminent men, to which the human race is subject, and this is largely increased in proportion to the general insalubrity of a country; here it sometimes constitutes more than three-fourths, and if to this is added the whole zymotic class, it is much greater. The more or less prevalence of this class, forms the criterion by which to estimate every where, the greater or less advanced condition of every country in salubrity. It is, upon this then that mainly depends the permanent prosperity of every community, and as the advancement of true scientific medicine is identified with this, it is clearly our interest to promote it. Enlightened interest and duty are one; it becomes our sacred duty then, not only to ourselves, but the public, to dive into those mysteries of nature; trace out the causes of these maladies, which, otherwise will be forever hidden from the indolent, and unknown to the ignorant. Let us investigate and interpret the laws of their origin and production; follow out effects to their causes; “mount to their antecedents;” look through nature up to nature’s God. Influenced by this spirit, the proper interpretation will not be withheld from the industrious and persevering inquirer. We live in an age pregnant with mighty events; (I mean, of course, in philosophical investigation;) the human mind seems to have cast off its trammels; a strong impelling force is forever impelling us onward; “un pois invincible nous entraine, it faut toujours avancer;” and although we may look back as the traveller on the desert, *it is only to see if we are travelling straight onward*, and not diverging to the right or to the left; that is the true law of philosophic investigation.

Life has been denominated by some of our most profound thinkers, and concurred in by others, “a forced state;”\* that is, without the influence produced by stimuli, the first impulse of being would be wanting, and the capacity would become extinguished for want of development. The Egyptian grain of wheat buried for thousands of years, and enveloped in the cerements of the dead, becomes vivified on the application of heat and moisture; such too, are the conditions required to sustain the action of healthy human beings. But this is not all. God made man perfect,

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\* Hunter, Rush, etc.

although "he has sought out many inventions." Man is not now as he came from the hands of his maker. Disease has resulted from the infraction of nature's laws; they are no longer in harmony with his being; the elements are at war with his existence. But a kind Providence has bestowed upon him the means of partially, at least, remedying this condition, and restoring him measurably to primeval longevity; and it is through the cultivation of that God-like mind by which we trace our being, and almost know that we are a part of himself in some of the attributes he has bestowed upon us. It is through this we read his *hand-writing* throughout his works; it is through this we trace the harmony of his laws; and it is through these we must apply the remedy to the defects brought on us by our own willful negligence and ignorance.

To apply these remarks to the subject before us, let us investigate the influences which affect the health of man in the aggregate; study those which curtail his life in different climates, countries and conditions "of more than half his years." Upon the present occasion, it would detain you too long to attempt to embrace all these within the narrow limits to which this address is necessarily restricted; let it suffice then, for the present, to limit ourselves to some of those climatic conditions which influence the vital laws here.

I present before you the Meteorological elements or Climatic condition of this locality for the last year, in a Chart, as being much more easily taken in by the eye, and more readily comprehended by the mind. The upper portion represents the climatic elements of direct SOLAR AND SHADE TEMPERATURE; the DEW POINT AND RAIN, during each month of the year.

This chart fully confirms what has been said in another place, in relation to our climatic condition, and the sanitary influences attributed to it. *High solar radiation* above the mean, has, as usual, produced disastrous effects on health. The amount of *rain* that has fallen, is actually more than an inch over the average of the last seven years — although less than fell in 1853, by near ten inches, but the peculiarity has been in its distribution. September being this year the sickly month, more rain has fallen during it, than during the last twenty years; this is a uniform result. During November and December, not much more than half has fallen of the average of the last seven years.

On the lower part, I have endeavored to represent the vital, or rather, the mortuary aspect. I interpret these as cause and effect; they are con-



stant, and with trifling variations they are universal; there is no exception known to me, *unless the malarious condition* should be absent, and in this place that is rarely so. The proof of this is contained in the series of charts I have been in the habit of making for a number of years back — indeed, as far as materials could be obtained so as to enable me to understand our true climatic condition. The proof is positive and demonstrative of the positions assumed.

It has been stated in another place, that the descent of the dew point to near sixty degrees, puts an end to yellow fever as an epidemic; that this was the result of a long series of years of observation; that this was the average, and that it varied but a few degrees from it in any year, such was its remarkably uniformity. It is unreasonable to attribute this to accident, and hence, I conclude it is a *law of the disease*. Its application to other climates is in progress and I hope to be able to apprise you of it at a subsequent meeting. This law has been most fully confirmed during the late epidemic.

In relation to cholera it was stated, that although there existed a greater range of climatic conditions, it was believed they were understood, and that its occurrence was confined to certain limitations. These views have been strongly corroborated during the late cholera of November and December, (and were to the present date,) and particularly in relation to what was said of the “drying power.”

As every step we take, bringing within the domain of the certainties of science, any knowledge of the afflictions to which our race is subject, tends to the true advancement of our profession, I have now the gratification of calling your attention, in the farther investigation of our climatic condition, to another still more fatal malady, together with an exposition of the meteorological elements coincident, if not causing it. I mean *coup de soleil* — “sun stroke,” as it is improperly called — probably one of the most fatal diseases to which man is subject, excepting hydrophobia.

You will recollect that in the early part of last summer, there occurred in this city an unprecedented number of cases of this malady. Let us see what were the meteorological elements that were most active at this period. Whatever may have been the opinion of the profession in relation to the climatic conditions accompanying or causing the two former diseases, (yellow fever and cholera,) it has not fallen to my lot to hear any one doubt as to the influence productive of this — with great unanimity ascribing it to exposure to the *direct rays of the sun*. This is unquestionably a great mistake,

for I have witnessed the direct solar temperature twenty, thirty, and even forty degrees higher without the occurrence of a single case. And so, the temperature in the shade alone, is often higher without this effect resulting. But, here we had both the temperature in the sun and shade too, at near their highest points, (not always *pari passu*,) and a *dew point* and a *temperature of evaporation at their very highest points we have never witnessed in this country*. The dissolving power of air and capacity for moisture, was unprecedentedly great—of course, accompanied with a great relaxation of the system and of a power of vital resistance at its minimum. It was just there that the direct temperature produced its lethiferous influence. In direct presumption of the correctness of this reasoning, it is well known that this malady is almost, if not entirely confined to intemperate subjects, with low vital power.

The most remarkable climatic conditions in relation to cholera, is the fluctuating character of the elements influencing it, and more particularly, of the “drying power.” This has been eminently realized on its every occurrence in this country, and particularly, last year, here and in various parts of the United States; with that exception, the present winter has been as remarkable for health as it has been for dryness.

Probably the most injurious influence, in this country, is the excess of moisture to which we are exposed. The exact amount requisite for health is not known; of course it varies under different temperatures and climates. Baron Humboldt remarked of Cumana, in South America, that it was the hottest, *dryest*, and *healthiest* city in tropical America. If, then, we proceed to a different temperature and climate and witness the remarkable health enjoyed, as in England and Holland, enveloped in their perpetual fogs, and then notice the great difference in the health of the same people emigrating from that temperature, which is very low, to this which is very high, and we actually know, from experience, that the fatality attending their immigration here is greater than what is attendant on any other.\* It will be clearly apparent that moisture at high temperatures, and moisture at low temperatures, is about as different in its influences on the human constitution as the pressure of the atmosphere is on his frame at different densities. In relation to this it is well known that on ascending a lofty mountain, we soon become deprived of our strength, in proportion as we ascend; fatigue is felt on the least exertion, and we become liable to hemorrhages from the delicate membranes, no longer experiencing

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\* Vide table at page 248, Report of N. O. Sanitary Commission.

the sustaining power of the atmosphere, becoming ruptured. While on the contrary, the *opposite condition* shows the bracing and ever cheering influence of condensed air on the system. It has been found that "a person so exposed breathes with increased facility; he feels as if the capacity of his lungs was enlarged; his respirations become deeper and less frequent; he experiences, in the course of a short time, an agreeable glow in his chest, as if the pulmonary cells were becoming dilated with an elastic spirit, while the whole frame receives, at each inspiration, fresh vital impulsion; the functions of the brain are excited; the imagination becomes vivid; the ideas flow with delightful facility; digestion is rendered more active, as after gentle exercise in the air, because the secretory organs participate immediately in the increased energy of the arteriel system. These experiments were made on persons in a mine in France, where men worked under a pressure of three atmospheres. Upon many of them the first sensations were painful, especially upon the eyes and ears; but, ere long, they became quite reconciled to the bracing element. Old asthmatics become effective operatives, deaf persons recover their hearing, while others are sensible to the slightest whisper. The latter phenomena doubtless proceeds from the strong impulses of the dense air upon the membrane of the drum of the year. Men who descend to considerable depths in diving bells, experience a considerable augmentation of muscular energy; it infuses into their muscles such power, that they can easily execute double the work without fatigue, which they are unable to execute in the open air; they thereby acquire the power of bending on their knees strong bars of iron, which they would find quite inflexible by their utmost efforts, when drawn up to the surface."\*

I have given these remarkable instances of the influence of these prominent meteorological elements when in excess on the human frame, as they will aid me in illustration of the conclusion I have come to in relation to the subject under consideration, as they tend to show the susceptibilities of our constitution to them. This conclusion then is, that it was through the combined agency of the extremes of temperature direct and indirect, and the intense humidity, on individuals of diminished vital resistance, that produces that fatal malady called the "*coup de soleil*."

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\* Vide Report to the State Medical Society, by the Author. A few weeks after the discourse was delivered, I had the great satisfaction of receiving from Professor C. A. Lee, a remarkable confirmation of these opinions, from actual experience in New York; and all who are acquainted with his long duration to the study of climatic influences, will receive it with due credit.

The explanation of their *modus operandi*, is not at all obscure or even doubtful. With a dew point at eighty or more, the evaporation from the surface is at its minimum, with a stagnant atmosphere and high temperature in sun and shade; the enfeebled secretory organs are now almost in a state of collapse; the body is bathed in perspiration; the system is replete with retained effete and worn out materials; the energies of life are at the lowest stage, and with those of bad habits, there exists a predisposition to fatal congestions, and the brain and lungs throughout, which circulate the largest amount of blood, become their seat; and as Dalton has shown from his experiments on the relative evaporating power of the lungs and surface, that the former gives off nearly three times as much as the latter, when in a normal condition; it fully accounts for the congestions when this source is cut off, which we almost uniformly find here on dissection from those dying of what is denominated *coup de soleil*. And again, as the process of evaporation is the great, if not the only refrigerating operation of the body, and as this is reduced to its minimum here, the remarkable heat with which bodies so struck, are now effected, is also satisfactorily accounted for independent of any influence of the nervous system.

These conclusions are farther borne out and corroborated by its rare occurrence within the tropics, where direct radiation is less than at the North, as well as the indirect (or shade temperature,) for a certain time. In crossing sandy deserts we find two of the constituents to produce it, (direct and indirect heat,) but not the third, (humidity,) and consequently it is very rare, if it ever occurs. In the Northern cities, it is much more apt to occur than at the South, from the presence of the combination of the three agents to which I attribute its liability, and accordingly it is much more common there than here. The temperature in the sun at the North, is every year greater than it is here — this is essential, and when the other agencies are present with the constitutional liability, it is of frequent occurrence. It may be said that this is the weather to which has been ascribed the occurrence of yellow fever. It is not so. Yellow fever rarely occurs with a thermometer in the shade, in a fair exposure, over eighty-eight degrees, and usually several degrees lower. It is characterized by being hot in the sun, and *cool* in the shade at the same time. With *coup de soleil*, both are hot and with a suffocating atmosphere; the one is apt to occur early in the season and suddenly; the other late, or after a certain duration of many weeks; nearly two-thirds of those occurring here last year were during the last week in June, and during that month and the



succeeding, more than one hundred more cases of this malady occurred than of yellow fever. It rarely occurs to any extent while yellow fever is raging, clearly demonstrating that it depends for its causation on different physical conditions. So far as I have been able to ascertain, the above expresses them.

Different climates have different forms and modes of morbid action; certain quite definite climates have special diseases quite as definite; there are geographical limits to each. These have been specified with much minuteness in the report to which I have invited your attention, and need not be repeated here. In my mind, I cannot separate these as cause and effect. Climate, whether natural, or such as God has made it—or *artificial*, such as man has changed it, has results, which are written in every page of man's history. He who would gather wisdom for present use, or future guidance, must do it by consulting the past; it is full of instruction; deeply—deeply, have we paid the penalty by refusing to listen to its lessons.

To correct errors, as I have said before, we must first understand them; to become thoroughly acquainted with what is meant by climate, we must experiment; we must study its details; we must see and comprehend its workings; we must have before our eyes its influences on our race; that it is a real tangible thing. We must endeavor to show how this is effected through the physiological system; or, system of vital laws; to comprehend how the pathological effect is produced. When this is once admitted, let us turn to the overwhelming influence by which the great mass of mankind reach their permature graves, and see what these influences consist of; analyze them; see if it is heat, plus, or minus, direct or indirect; humidity more or less; pressure of the atmosphere; electricity; terrestrial magnetism; filth, or some additional agency not yet understood. How far these act alone; or what are the conditions in combination with which they are influenced; what circumstances these are, and *how* they produce their effects on the system. Natural climates, or such as they come from the hand of our kind Creator and Benefactor, are rarely alone injurious to the health of man; it is the artificial condition, or combination by which this is all altered or tortured, by which he suffers. It is an old saying, that "God made the country, and man made the town;" the difference in their mortalities of nearly forty per cent. shows the difference of their origins, and it is the *mode of making this* which is so fatal to human health and life. Avarice, want, ignorance, produce crowding and filth, bad

ventilation and humidity, and the inevitable resultant — disease, misery and death. The investigations of statisticians have clearly shown, that cities cannot increase in number by their own natural growth; they would become depopulated from the causes just enumerated, but for immigration from the country. For instance, the actual *nativities* of the great city of London, in a northern climate, and remarkably healthy country, scarcely exceeds one-fourth of its population! In this city, our *nativities* have not been precisely ascertained, having been embraced, by the census takers, with those deriving their births from the entire *State* of Louisiana, and amount only to about the same number; the actual *nativities* here would be much less.

I suppose I shall be deemed an enthusiast when I express my belief, that when all the meteorological elements that influence man shall have become well understood, that the prevalence of each of the *great classes* of maladies will be known to the accurate meteorological observer, on the inspection of a proper record of them, always presuming that the terrene or malarious conditions shall be the same. For this to be fully understood, it is essential that something more than a mere inspection of the mortuary record be furnished. To make this at all applicable, allowance must be made for the difference of time between the inception of the impression and the result in death. In some countries it is made the duty of medical men, under heavy penalties, to furnish to the authorities the date of the occurrence of *every case of disease*. In this country it would be impossible to enforce this; it has to be left to the zeal and love of science of the true student and to public institutions. Besides this there is another difficulty. Few devote themselves to keeping meteorological records with that minuteness and extent essential to the accuracy of the experiments. These are difficulties which are easily surmounted by an adequate amount of zeal and sincere desire to understand the principal causes of human maladies. I feel the most thorough conviction that we neither perform our full duty either to the public or profession, without this knowledge. Our eminent countryman, Dr. Rush, although little was known of meteorology at the period he flourished, was so impressed with the value of the *habit of mind* which it inculcated and taught, in making observations and a close study of contemporaneous phenomena, that he recommended keeping a journal of the weather, were it for this alone.

In conclusion, gentlemen, permit me to invite your serious attention to this important department of etiology; rely upon it, that little instrument

(the thermometer) is its true exponent, through thermal and hygrometrical laws. Nor is its value confined here. By it, the great Gulf stream between this Continent and Europe was first discovered, with its limits and extent, with all its important commercial and political bearings; by an investigation of whose laws, influences and currents, and congenerous circumstances, one of our distinguished countrymen (Lieut. Maury) has made himself immortal, in conferring unmeasured benefits, not only on his own countrymen, but upon all nations. By it, the mariner first discovers his near approach to land. By it, and its coadjutors, more certainty and science are given to agricultural pursuits, and by it, the true climatic relations of all animal and vegetable creation is made known. Indeed, in all these important aspects, it is the true interpreter by which an all-wise Creator makes known, by secondary causes, his great laws to man.

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## Operations for the Formation of Artificial Pupil.

*Reported by DR. C. BEARD.*

Thomas McCabe, native of Ireland, 43 years of age, entered Charity Hospital in July, 1853, to be treated for yellow fever. He had been sick ten days, and soon after his admission threw up black vomit, and bled profusely from gums, nose and conjunctiva. His condition was thought to be hopeless, and in the bustle of the crowded ward, but little attention, comparatively, was given him. A severe purulent ophthalmia ensued, possibly from want of attention to cleanliness. He remained under medical treatment nearly a month, when he was transferred to the eye wards. At this time his condition was deplorable; he was covered with furuncles — so frequently observed as sequellæ in the yellow fever of 1853 — was much emaciated, completely blind, and suffering intense pains from a sub acute inflammation of the eyes, accompanied with a muco-purulent discharge and violent photophobia. The damage which had been done by the inflammation could not be ascertained, as the condition of his eyes forbade rigid examination. The patient was ordered a good diet, with wine astringent collyria, with counter irritants to temples, and frictions of mercurial ointment and belladonna to orbit.

Under this treatment the pain gradually diminished, photophobia ceased, and an examination revealed the following conditions of the organs: Left

eye—conjunctiva red, thickened, and somewhat granular; cornea completely leucomatous, slightly vascular, and flattened; sight completely abolished. Right eye—similar condition of conjunctiva; cornea affected with leucoma, extending to within a line and a half of its periphery, somewhat irregular in form, and presenting near the center a dark cicatrix, the point at which a hernia of the iris had protruded. The capacity of the anterior chamber was reduced about one-half by adhesion of the iris to its posterior surface. With this eye the patient could barely distinguish daylight from darkness. An alterative treatment, consisting of small doses of calomel, with occasional purgatives, and stimulating collyria, was followed, in two months, with considerable benefit; the opacities of both cornea sensibly diminished at their peripheries, and with the right eye the patient could discern the shadow of the hand passed before it, while the left cornea presented a transparent zone at its circumference about a line in width.

I operated on the left eye in October, 1853, in the following manner: The patient being in a recumbent position, and the eye lids separated by an assistant, the point of a cataract knife was passed through the center of the cornea in such a way as to make a verticle slit, the point of the knife being directed downwards and outwards, so as to form a valvular opening in the cornea and prevent a premature escape of the aqueous humor. A fine hook was then introduced through the corneal opening, the iris caught at its external border and detached, traction made, the hook withdrawn with a portion of the iris, which was allowed to remain fastened in the wound. The patient's eye had been accustomed to the touch of instruments a few days anterior to the operation and there was no difficulty in retaining it in position. No hemorrhage occurred, and the patient did not complain of pain. The eye was closed with strips of court plaster, and the patient placed in a bed secluded from the light; a dose of opium ordered, together with iced applications, which were to be frequently renewed during the first twenty-four hours. Not the slightest inflammation occurred, and at the termination of four days the patient was up in the ward.

One month subsequent to this I operated in a similar manner on the other eye. Slight hemorrhage occurred in tearing the iris from its ciliary attachments—the blood rendering the aqueous humor somewhat turbid.

The present condition of the patient is highly satisfactory. Absorbtion of the lymph has gone on in the opacities to a considerable extent, and



the transparent zone has increased in width in both eyes. In the outer portion of the left iris is a triangular pupil, the base of which is a line and a half in extent, and corresponds to the ciliary border of the iris; the apex of the pupil is lost in the opacity, its size is somewhat greater than natural, but its usefulness is diminished by the leucoma which overlaps it to about one-third of its extent; this opacity is gradually diminishing.

In the right eye the outline of the new pupil is not so well defined, it is not so large, and is slightly obstructed by one or two shreds of partially opaque lymph. The patient is affected with double convergent strabismus, which is only observable when the eye is fixed on one object.

Sight at present. The patient sees equally well with either eye, and what is somewhat singular—where both eyes have been operated on—better with both than with one. He can read the headings of newspapers, see the hands of a watch, distinguish faces, and perform many offices, useful to himself and others.

Henry Beck, aged 36 years, native of Adams county, Miss., rather delicate constitution, inclined to phthisis pulmonalis, entered the eye wards of the Charity Hospital, under my charge, on the 20th of June, 1854.

The condition of his eye was as follows: Right eye—conjunctiva and lids healthy, but bearing marks of previous inflammation; cornea transparent—slightly elongated in its horizontal diameter, otherwise natural; iris discolored and studded here and there with small flakes of lymph, and receding at its pupillary margin; pupil much contracted, completely obstructed by a deposit of lymph, and evidently adherent to the capsule of the lens; sight completely abolished.

Left eye—same appearance of lids and conjunctiva; anterior chamber somewhat more capacious than natural; cornea as in right eye; iris discolored; pupil displaced, ragged, and dragged downwards and outwards, probably by bands of lymph, and situated nearly at the ciliary margin of the iris. This was completely obstructed by shreds of opaque lymph, and what appeared to be portions of an opaque lens. The anterior chamber was diminished in size, the whole organ considerably softer and somewhat smaller than its fellow; perception of light very faint.

Beck's history, previous to the time of his entrance into the hospital, is as follows: In the fall of 1847 he was affected with chancre, which became indurated, and was followed some six or eight weeks afterwards with alopecia, roseola, and swelling of the inguinal glands. About February, 1848, he first noticed that his left eye was affected, his sight became

less perfect, and he had supra orbital pain, lachrymation, photophobia, and probably some conjunctivitis. A month subsequent to this his other eye became affected in a manner similar to the first. Vision gradually diminished in both eyes until at the end of a year he was totally blind. He was under treatment at various times previous to his entrance into the hospital, more for the relief of pain than with the hope of improvement to his sight.

At the time of his entrance into the hospital all signs of inflammation had disappeared. A month after I first saw him, I operated on his right eye by passing a straight cataract needle, sharp at both edges, through the sclerotic, as in the operation for cataract, endeavoring to break up the adhesions which had formed between the iris and capsule; in this I entirely failed. No inflammation followed, however, and there was no harm done. A few weeks subsequent to this I attempted to make an artificial pupil in the same manner as in the case of McCabe. The pupil was formed, but behind it the capsule of the lens was seen to be perfectly opaque; a slow inflammation followed this operation and destroyed all hopes of success for any future attempt in this eye.

The left eye, apparently that which promised the least success from any operation, was now attended to. Four attempts were made during the next six months to remove the adhesions which occupied the displaced pupil, but with no success. I then determined on making an artificial pupil, hoping that the lens, which was probably opaque in the eye, had been destroyed by the previous operation. An operation was accordingly performed, as in McCabe's case, and a pupil made at the inner and upper portion of the iris. The patient had no bad symptoms and was up in three or four days. The eye was extremely sensitive to light for two or three weeks after the operation, and the patient could only distinguish a shadow when objects were passed before his eye. The photophobia gradually decreased, however, and the new pupil, of a size somewhat larger than the natural, well defined, black, and unobstructed, began to fulfill its functions in a most satisfactory manner. At this time, nearly two months since the operation, the patient can see remarkably well, the previous condition of the eye being considered. With a concavo-convex glass he can conduct himself, see large objects, and at a distance of ten or twelve feet recognize persons. With the periscopic glass, together with a strong bi-convex glass he can read one-sixteenth of an inch type and see the hands and figures of a watch.

*[To be Continued.]*

*Adversaria Medica.*

## No. III.

*By I. L. CRAWCOUR, M. D., Visiting Physician, Charity Hospital, New Orleans; Honorary Corresponding Member of the Society of Arts, of England, etc., etc.*

## ARSENIC — ITS DETECTION.

At the present moment, when a case of homicide by arsenical poisoning is undergoing investigation before our tribunals, it may, perhaps, neither be uninteresting nor useless, to examine in a succinct manner, the various modes by which the detection of this substance is ensured; and fortunately for the ends of justice, few poisons so readily yield indications of their presence, as the one under consideration. The tests for arsenious acid, or white arsenic as it is commonly called, are simple and free from fallacy; but as it is rare, that for the purpose of juridical inquiry, we have it presented in an uncomplicated state — various processes have been devised for its liberation from the tissues with which it is combined, and which so frequently mask its re-actions.

In its simple state, arsenious acid is a white powder, readily subliming at a gentle heat; in its state of aqueous solution, depositing a canary yellow precipitate on the addition of ammonio-nitrate of silver, and a grass-green one on the addition of the ammonio-sulphate of copper, both these precipitates being soluble in most of the acids and in ammonia, insoluble in solutions of potash or soda — heated in a tube with a little powdered charcoal arsenious acid is decomposed, and metallic arsenic results as a brilliant brownish or greyish coating on the sides of the tube, and at the same time, a peculiar odor resembling garlic is perceived; by gently heating this ring of metallic arsenic, it again combines with oxygen — is re-formed into arsenious acid, and a series of brilliant crystalline points of an octohedral form, resembling the deposits of oxalate of lime, so frequently found in the urine, take the place of the metallic sublimate. The crystals may be dissolved in a little distilled water, and tested either by the ammonio-nitrate of silver, or by the ammonio-sulphate of copper, when the re-actions described above will take place. An advantage possessed by the precipitate produced by the ammonio-sulphate of copper is, that the resulting arsenite of copper may be re-introduced into the reduction tube, and heated, when metallic arsenic will again sublime, and metallic copper be left at the bottom of the tube. By these tests, we conclusively prove the presence of arsenic, and that without any fallacy to throw the slightest

doubt upon the proof. Formerly, much stress was laid upon the alliaceous odor, and upon the crystalline form of the deposit; but, to the former, are many impediments and fallacies — thus, the presence of organic matter, such as flour, will entirely mask the odor, while phosphorus, many vegetable substances, and a compound of albumen and fat, will yield the garlic odor as readily as arsenic itself. With regard to the crystalline form the salts of antimony are isomorphous with those of arsenic, and therefore, on this test alone, no reliance can be placed. Another mode, and possibly in the examination of organic liquids, one of the surest and most reliable, is the transmission of sulphuretted hydrogen gas through the solution acidulated with either muriatic or nitric acid. A precipitate of a rich yellow color, (the sesqui-sulphuret of arsenic,) will immediately fall — this is soluble in solution of ammonia — insoluble in muriatic acid. This test is of extreme delicacy, for according to Brande, the yellow tint is observed when a ten thousandth part of arsenious acid is present, and *the yellow precipitate* is visible in an acidulated solution of one part of arsenious acid in eighty thousand of water. (Brande's Manual of Chemistry, p. 915.) This precipitate is known to be sesqui-sulphuret of arsenic, by the following tests: it is insoluble in all acids, mineral or vegetable; it is soluble in solutions of soda, potash and ammonia, and on being dried and heated with an equal quantity of cyanide of potassium, in a reduction tube, it is resolved into metallic arsenic. The only substance that at all resembles arsenic in its re-action with sulphuretted hydrogen, is cadmium, which also, precipitates as a golden yellow sulphuret — this precipitate, however, is soluble in muriatic acid — insoluble in ammonia, by which it is readily and easily distinguished from arsenic.

In judicial investigations, however, the arsenic is combined with organic substances, and the above re-action, though useful as confirmative, are useless as indicative tests. We have to devise means to set free the arsenic from the tissue with which it is united, and for this purpose, many processes have been devised. The earliest, and for many purposes, one of the best, is that known as Marsh's test, which depends for its efficacy upon the property possessed by nascent hydrogen of separating metals from their combinations, and precipitating them in the metallic state. The process is as follows:

The suspected liquid, acidulated with either sulphuric or muriatic acid, is poured over some pieces of pure zinc previously introduced into a proper apparatus. If no arsenic be present, hydrogen alone is the result; if



arsenic exists, arseniuretted hydrogen results, which may be ignited and recognized by the following characters :

1. It burns with a bluish white flame, evolves a whiteish smoke, and exhales a garlic odor.

2. If the flame be allowed to play upon a piece of glass or white porcelain, a brownish stain is produced, usually surrounded by a white film — this is metallic arsenic, surrounded by a ring of arsenious acid.

3. If the flame play for a few seconds upon a piece of glass or card, moistened with ammonio-nitrate of silver, the canary colored arsenite of silver is immediately produced.

The apparatus invented by Mr. Marsh for this experiment, is so well known, as to need no description, but an ordinary bottle fitted with a cork perforated by a glass tube, will answer every purpose.

The objections to this test, are the frothing of organic liquids by which it is forced up the tube, and the consequent difficulty of igniting the gas ; the loss of arsenic by escape of arseniuretted hydrogen, which, when the quantity is small, is an item of great importance — the danger and frequency of explosion, by which all the products are lost, and frequently the experiment defeated — and lastly, the similarity between stains produced by antimoniuuretted and arseniuretted hydrogen.

The frothing of the liquid may be, and usually is in practice, prevented by charring the organic substance. This, however, owing to the high temperature necessarily employed, produces a certain loss of arsenic. The process usually followed, is that of Messrs. Danger and Flandin, which is as follows :

The organic substance is mixed with concentrated sulphuric acid, and heated until vapors of sulphuric acid appear. Carbonization is speedily effected, a small quantity of nitric acid is then added, by which the arsenious acid,  $\text{As O}^3$  is converted into arsenic acid,  $\text{As O}^5$  (the more soluble salt,) and the whole evaporated to dryness ; it is then boiled in distilled water — filtered and introduced into the apparatus, as above described.

The objection I find to this process is the high temperature necessarily employed. Arsenious acid rises in vapor at about four hundred and twenty degrees F. Sulphuric acid boils at about six hundred and twenty degrees F., or nearly at a red heat. It is evident, therefore, that there must be a certain loss of arsenic, and where the metal exists in but very minute quantity it may be entirely dissipated. I have found that if nitric acid be substituted, the charring is effected at a much lower temperature, is

more rapid and the result in every respect more satisfactory. The strongest nitric acid of sp. g. 1.50, boils at 210 degrees, or below the boiling point of water, that of sp. g. 1.42—the pharmacopœal strength, boils at 268 degrees, or a little above that of boiling water, in every instance far below the vaporizing point of arsenic. The suspected substance is to be boiled with the strongest nitric acid, added in just sufficient quantity to moisten it thoroughly; after a short period it entirely dissolves. Water may then be added by slow degrees, and the whole, now clear solution, be gently evaporated. As soon as fumes of nitrous acid begin to appear the vessel may be removed from the source of heat. Evaporation will then go on to dryness or a slight deflagration may take place, when a friable carbonaceous mass will be left, which may be reduced to powder, boiled in water slightly acidulated with muriatic acid and filtered. This is introduced into Marsh's apparatus and treated as above.

By this means I believe all loss of arsenic will be prevented. But the strongest objection to the use of Marsh's process is the loss of arseniuretted hydrogen and the risk of explosion. We are told in books, after the introduction of the ingredients to wait until all air is expelled before igniting the gas. It is extremely difficult to know when all the air is expelled, as the period will vary according to the rapidity with which hydrogen is generated and with the size of the vessel in which it is generated. I have sometimes waited more than five minutes to allow of the escape of air, and after applying the light, been rewarded for my patience by an explosion, which has scattered and entirely wasted the liquid to be examined. On the other hand, if we wait too long we may lose all the arseniuretted hydrogen. Again, when the evolution of gas is slow, it is sometimes very difficult to procure a flame at all. Taking it all in all Marsh's process is not only tedious but difficult. To obviate some of these difficulties I propose the following apparatus as a modification of Marsh's original plan, and I have found, in practice, that it works remarkably well. It is difficult, however, to describe it without a diagram.

Into a wide mouthed bottle, which I will call A, let a good cork be fitted, and to this cork be adapted three glass tubes, one — B, curved at right angles and one leg reaching to the bottom of the bottle. Another — C, straight, reaching about three parts down; and another — D, merely piercing the cork, terminating above in a capillary orifice. Into this bottle the fluid to be examined, slightly acidulated with muriatic acid, is introduced. The tube B is connected with an ordinary gas generating bottle

containing fragments of zinc and muriatic acid. Hydrogen gas is instantly evolved which is allowed to traverse the fluid in B until all air is expelled. As there can be no loss of arsenic, we can take our time about it. When we are sure that all air is expelled, the gas may be lighted at its point of escape, D, and while burning, small fragments of zinc may be dropped through the tube C into the arsenical fluid. Nascent hydrogen in very minute bubbles will be evolved, the arsenic decomposed and this uniting with the hydrogen from the gas bottle and already burning, will deposit the arsenic on a slip of glass or porcelain held above the flame.

By this apparatus, we avoid all risk of explosion; we ensure a full and continuous current of hydrogen; we prevent any waste of arsenic and all frothing, and the result is rendered more striking by the contrast between the flame of the pure hydrogen, and that evolved after the generation of the arseniuretted gas.

The fallacies of this test have been over-estimated in my opinion—too much stress laid upon the difficulty of distinguishing between arsenical and antimonial stains. The differences are so great and distinct, that I cannot understand a single case in which they could be confounded.

The arsenical stain may be distinguished from the antimonial, by the following tests:

Arsenical stains are soluble in nitric acid—if this be gently evaporated to dryness, the resulting arsenic acid is soluble in water, and gives with nitrate of silver, a brick-red precipitate. Antimonial stains similarly treated, are not soluble in water, and give no precipitate on the addition of nitrate of silver.

Arsenical stains exposed to the vapors of phosphorous, entirely disappears—antimonial do not. I have found, that an arsenical spot exposed to the vapor of phosphorus, requires about eight hours for its disappearance; this test, therefore, would be inapplicable where a distinction was immediately wanted.

Arsenical stains are soluble in solution of the chlorides—antimonial not.

Iodate of potash in concentrated solution, turns arsenical stains of a brownish red, and dissolves them rapidly; it has no effect on those of antimony.

The action of bromine, is also very characteristic. If a drop of bromine be placed on a saucer, and a capsule containing an arsenical spot be inverted over it, the spots take a bright lemon tinge in a short time. Antimonial

spots are acted on more rapidly, (in from ten to fifteen seconds,) and take an orange tint. Both become colorless if exposed to the air, and the color is again restored if treated with a strong solution of sulphuretted hydrogen. If to this secondary yellow, ammonia be added, the color disappears if the spot be arsenical; it is unaffected if it be antimonial. This test, not only affords an excellent mode of distinguishing the one from the other, but also of separating them, if, as sometimes occurs, both be present. Perhaps, after all, the most distinguishing characteristic, and the one most easy to observe, is the action of the flame upon a slip of glass, wetted with ammonia-nitrate of silver, the yellow arsenite of silver is instantaneously produced. This should never be omitted, as no other substance but arsenic, can by any possibility produce it.

In convenience and rapidity however, few tests are equal to that of Reinsch, which consists in the reduction of the metallic arsenic upon slips of copper boiled with the suspected liquid, previously acidified with muriatic acid. This test is of extreme delicacy, and Reinsch himself states that by it the one hundred thousandth part of a grain may be detected. I have observed the deposit of metallic arsenic to take place upon copper gauze when two drops of Fowler's solution, equaling the sixtieth of a grain of white arsenic, were diffused through two ounces and a half of coffee. In this case the arsenic formed only the seventy-two thousandth part of the whole.

The slips of copper coated with the arsenic are removed from the solution, washed and dried, introduced into a clean and dry reduction tube, (an ordinary German glass test tube will answer every purpose,) a gentle heat is applied, the metallic arsenic sublimes and condenses on the side of the tube in minute octohedral crystals of arsenious acid, which may be dissolved in a little water and tested by the ammonia-nitrate of silver. When this latter precaution is observed, this test is entirely free from fallacy. Without this, however, the mere deposit is of no value, for many metals, such as antimony, tin, lead, zinc and bismuth, will give a similar appearance to the copper.

In the course of some experiments upon the most ready mode of detecting arsenic in organic substances, I have found the following process exceedingly valuable, as by it we combine the modes of Reinsch and Marsh; its application is very easy and by it very decided results are obtained, which are confirmatory one of the other. The tissue to be examined (say the liver) is to be boiled with just sufficient nitric acid to dissolve it. This solution is then diluted with water and the boiling continued for some time.



We now obtain a clear greenish yellow solution containing the *whole of the substance to be experimented on*, this being of great value where we are led to suppose that the poison exists in very minute quantity. To this solution are added some slips of copper gauze and some fragments of pure zinc. As long as the copper is in contact with the zinc no destructive action takes place, and the whole is boiled for a few minutes. The steel grey deposit of arsenic will become visible upon the copper in a very short time. The copper is then very carefully washed to free it from adherent zinc, dried and introduced into the reduction tube and the arsenic converted into arsenious acid. The zinc also becomes coated with metallic arsenic, and may be introduced into Marsh's apparatus when arseniuretted hydrogen will be evolved. The advantage of this process is its rapidity, the certainty of its indication and the freedom from frothing when tested by Marsh's apparatus, without the necessity of previously incinerating the tissue. When it is considered advisable to collect the whole of the arsenic I would recommend that the tissue be dissolved in nitric acid, as in the above process, and diluted with water, and a current of sulphuretted hydrogen transmitted through the clear transparent solution as long as any precipitate falls. This yellow precipitate, the sesqui-sulphuret of arsenic, may then be dried and weighed, and as it is a perfectly definite compound, the weight of the arsenic may be calculated from its atomic constitution.

It is often a medico-legal question as to whether arsenic when found in the body may not result from the medicinal use of the drug. From the experiments of Messrs. Danger and Flandin, it appears that it does not so accumulate, but is eliminated very rapidly. "M. Flandin administered to animals doses of arsenic mixed with their food, beginning with the one-sixty-fifth part of a grain, and he found that after an interval of nine months the animals might be made to take upwards of fifteen grains in twenty-four hours, *without injury to their appetite or health!* During this interval their urine was frequently analyzed at different intervals without any arsenic being discovered. Three days after the last dose of arsenious acid was given, the animals were killed and their organs successively analyzed, but no arsenic was detected in their viscera, flesh or bones!" (*Taylor on Poisons.*) As I was anxious to examine this subject, I gave a rabbit for a few days small medicinal doses of Fowler's solution, commencing with five minims twice daily; on the second day, ten minims were given twice in the twenty-four hours, and on the third day, fifteen minims; on the morning of the fourth day, twenty minims were given at

once, and about nine o'clock in the evening of that day the animal died. I examined the body the next day. The liver was filled with soft, yellow tubercles, and arsenic was readily detected in the liver, in the stomach, in the kidneys and in a portion of the intestines. The bladder contained about a drachm of urine which also gave unmistakable evidence of arsenic. I must confess I do not understand how fifteen grains of arsenic could be eliminated from the system in three days, and that without any being discovered in the urine. Iodine, a much more soluble salt than arsenic, and one much more readily eliminated, has been detected in the tissues of animals five days after they had ceased taking this substance. My experiments have, however, been too limited to allow of my offering any decided opinion on the subject.

The opinions of two distinguished French chemists would seem to be in favor of the accumulation of arsenic in the system when administered in frequently repeated and small doses. I quote the following from Messrs. Robin and Verdiel's *Traité de Chimie Anatomique*, page 505.

"On trouve quelquefois dans les os, des traces très faibles d'arsenic, et cela en si petite quantité qu'il est très difficile par l'analyse chimique, de le constater. La présence de l'arsenic dans l'économie n'a pas lieu d'étonner beaucoup, puis-qu'on sait que plusieurs sources d'eau contiennent de l'arsenic."

In the opinion of the same chemists, arsenic does not form a normal constituent of the body, and Orfila, who originally propounded this theory, soon after withdrew it; there was doubtless some fallacy in his mode of experimenting. With regard to antidotes to arsenic, we possess, probably, none that can be absolutely relied on; the hydrated sesqui-oxide of iron so highly vaunted, has proved to be unworthy of the confidence placed in it. The danger arrives not so much from the arsenic in the stomach, as from its constant solution in the juices, and consequently, from its circulation in the blood, and thus effecting the system. Some few years ago, it was discovered by Dr. Odling, now Demonstrator of Chemistry, at Guy's Hospital, that the sesqui-arsenates of many of the metals, were less poisonous than the arsenious acid itself, and that many of them might be swallowed with impunity. I subjoin an abstract of his experiments, as they are very interesting, and may not be generally known. He states, "that he was first desirous of discovering some compound containing arsenic, from the chemical relations of which an innocuous character might be deduced, and secondly, of ascertaining some means by which powdered arsenious acid in

the stomach, might be converted into this innocuous compound. He found that the sesqui-arseniates of iron and alumina, if not entirely harmless, were much less dangerous than ordinary white arsenic, when administered with an excess of the sesqui-oxide upon which the action of the gastric juice might be expended rather than upon the arseniate, or with an alkaline acetate, which, by re-acting upon the gastric secretion, would set free acetic acid, in which these sesqui-arseniates are insoluble, and the following experiments were tried. Some sesqui-arseniate of iron was prepared, and five grains mixed with ten of sesqui-oxide of iron, were administered to a rabbit. No ill effects were perceptible during a fortnight, and specimens of the fœces, passed during several days succeeding the administration of the arsenic, were examined by Marsh's test, and evidence of arsenic found."

In another experiment, ten grains of arseniate of ferric-oxide were mixed with ten of sesqui-oxide of iron, and five of crystallized acetate of potash, and administered to a dog. No ill effects were produced.

If in the stomach therefore, we can convert the arsenious acid into these sesqui-arseniates, we should render it comparatively harmless. Now, arsenious acid is readily converted by most oxydizing agents into arsenic acid, which, in the presence of the above mentioned bodies, (iron and alumina) from the insoluble sesqui-arseniates. The substances found most suitable for this purpose, were the chromates of iron and alumina, and a mixture of permanganate of potassa, with hydrate and acetate of ferric oxide, or with hydrate and acetate of alumina; the latter is preferred, in consequence of its gelatinous condition being particularly suitable for suspending the arsenic.

The aluminous salt, is made by precipitating hydrated alumina from the chloride by ammonia; this precipitate, after being well washed, is formed into an emulsion by agitating it with a concentrated solution of permanganate of potash, is mixed with this aluminous compound, and added to solutions containing arsenious acid; the whole of the poison is converted into the insoluble sesqui-arseniate of alumina, and if it be agitated with powdered arsenious acid, the same effect is produced, and solution entirely prevented. The following experiment was tried. Twenty grains of powdered arsenious acid were mixed with a portion of the antidote containing ten grains of permanganate of potassa, the whole agitated and filtered, and the precipitate, which contained the whole of the arsenic, was given to a dog. No ill effects were observed after a lapse of five hours from the time of exhibition, and during the night he vomited. In a subsequent

similar experiment, the vomited matters contained arsenic; in neither instances did the dog suffer any inconvenience.

These experiments which formed the subject of a paper, read by Dr. Odling, before the Physical Society of Guy's Hospital, in the year 1851, are exceedingly valuable, and as the substances forming the antidote are easily procured, they might be administered in every case where we suspect arsenic to have been administered. The effect is both mechanical and chemical, and they afford rational grounds for supposing that the continual absorption of the poison may be entirely prevented.



## Lectures on the Nervous System.

By MARSHAL HALL, M. D., of London.

### No. I.

At the request of the Medical Faculty of the Louisiana University, Dr. Marshal Hall, of London, delivered in that institution, on the 14th January, 1854, a lecture on the *Spinal System*, which was listened to by a number of medical gentlemen, and a large body of students.

The lecture was delivered in a style which was at once concise and clear. The following is but an imperfect outline of what was at the time uttered by the learned lecturer :

He commenced by stating that he had consented to deliver two lectures, and the present was to be devoted to an exposition of the Spinal System, especially of those views connected with it which he had so long labored to establish. The nervous centres, comprehending the brain and spinal marrow, have received the name of cerebro-spinal system. Now, this name he held to be a misnomer, the cerebral system and the spinal system being — as it was his special object to demonstrate — absolutely distinct.

When the head of an animal is removed, and the viscera extracted — as in the alligator before us — what were the more prominent results which would be observed to take place? We shall the better comprehend and answer this if we recollect, in the first place, that it is the peculiarity of the cerebral system, besides intellectuality, to give to all portions of the animal body sensation, and the power of spontaneous motion. When the head is therefore removed, in which these reside, the animal must necessarily lose all sensation and volition. It is true, there will be observed cer-



tain irregular movements in the several parts after this for a little time, but they soon cease, and are dependent on the irritation or shock produced by the severing of the nervous tissues. Having once ceased, no further movements are observed, unless on the application of some fresh cause of irritation, and the animal will in this position remain and die.

That the animal loses the power of both sensation and volition, when decapitated, was shown by the experiments which he had performed on the alligator in the presence of several of those now present, who were witnesses of the preceding day's experiments. When the head was removed and a little time given for the irregular movements above alluded to, to cease, the animal became perfectly still, and would in this position remain and die, if not interfered with. If it, in this condition, retained the power of sensation and spontaneous movement, it would not have remained perfectly still, as it did, after being placed in positions well calculated to excite both, supposing them to remain in the decapitated body.

But while in this position, in which, as stated, it would remain and die, if not interfered with, the application of any stimulus, such as pinching, served to excite convulsive movements anew, which also would soon cease until excited again. In cold-blooded animals of this kind these movements can be reproduced and observed for a longer period than in the warm-blooded ones. It was these irregular movements which some, when witnessing like experiments, have mistaken for manifestations of remaining sensibility and volition, and so interpreted them. They were in no degree, whatever, dependent on either of these, but were dependent on, and illustrative of, another and separate function, having its centre in the spinal marrow—and which he several years ago displayed, and named *excito-motor*. The parts separated from the brain, but still maintaining their connection with the spinal marrow, when irritated in any way will be found to exhibit, for a longer or shorter period, the phenomena dependent on this excito-morto power. The tail of the alligator, into which the spinal marrow is prolonged, can be made very clearly to illustrate this. Dr. Hall here pinched the extremity of the tail of the alligator, as experimented on privately the preceding day, when this motion was excited; and so of other parts when similarly irritated. On the cessation of the stimulus, all again was soon still.

He next referred to the views of Legallois and others of the older writers and experimenters, and stated in what particulars he differed from them, as well as in what particulars he differed from some of the Physio-

logists of the present day; and remarked that in so doing he, at least, showed that he claimed for himself the privilege of exercising freedom of thought and inquiry; but while doing so he ever would respect the exercise of the same right in others, and hoped, and expected that in this land of liberty, at least, where freedom of thought was guaranteed, this right would be freely accorded to him while amongst them.

By the aid of a few diagrams on the black-board, Dr. Hall entered next into the elucidation of spinal system and the excito-motor action, somewhat minutely — too much so for us to attempt to follow him throughout. He remarked that when we touch a peripheral nerve, this conveys an energy or impression, which, by this channel, reaches the spinal centre; from this centre another goes outward, which ends in the muscle moved, thus nearly forming a circle. To the completion of this almost circle we have, first, the surface itself effected; second, the *in-going* nerve, which has its origin in the surface irritated, and termination in the spinal centre; third, the spinal centre itself; and, in the fourth place, the *out-going* nerve, which has its course from the spinal centre, and its termination in the muscle. Suppose you destroy either the in-going or out-going nerve, or, leaving these intact, you destroy the spinal centre, the result, in either case, is the same; the spinal excito-motor nervous arc is destroyed.

If the spinal centre, for instance, be removed, motion attempted to be excited will be found to be entirely lost, because the central organ of the motion is removed, and so of the rest. The severing of any link in the circle, serves alike to destroy the integrity of the arc. I have shown, he said, four methods by which this can be destroyed. What we have here seen are the teachings of experiments, repeated and re-repeated on the lower animals — but which, for obvious reasons, cannot be repeated on our own species. Nevertheless, what we witness of the operations of Nature in one class of animals, will go a great way to illustrate her mode of action in others; there is a degree of harmony found throughout the whole.

It may be said that all this which he had been endeavoring to explain may be the true physiology of the nervous system, and yet of what use is this knowledge to those in practice. First, he would remark to this that nature never made any thing in vain; and such is eminently true of physiology. The act of procuring and taking food into the mouth is a purely voluntary one, dependent on the operation of the cerebrum; but the moment it enters into the pharynx it is beyond our control, and is entirely so during all the succeeding steps of deglutition, which are conducted under the

sphere of the true spinal system, as taught by him. If you divide the pharyngeal nerve the animal cannot survive long; but you may remove the whole of the brain without producing this result. A kitten, for instance, had this experiment performed on it. It lost all sense of external things, and spontaneous motion ceased. Food placed back in the pharynx was swallowed, this fact proving the independence of this function of the cerebral system. Without some means be adopted to excite to action the excito-motor system, no effect is produced. In the case alluded to, the presence of food in the pharynx acted as the stimulent, when the phenomena of swallowing were produced.

That the act of swallowing depends in these cases on the irritation produced by the contact of some substance with the branches of the in-going pharyngeal nerve, which irritation is conveyed to the spinal system, and reflected back by the out-going branches of the same, is by this means fully illustrated; and that this act, by a too frequent repetition, may so exhaust the excitant as to prevent a repetition of it. The act any one can prove on himself by making three successive efforts to swallow saliver. He will find it easy to perform the first act — the second will be accomplished with greater difficulty, while the third will not be performed at all, the excitant by this time being exhausted — try it. The action of snuff when applied to the nostrils, in producing sneezing, is precisely of the same character; and the same excito-motor action is perhaps still more strikingly illustrated by the effects produced by a drop of water falling into the larynx, which effects we cannot control, however anxious we may be to do so. What are the different parts employed in the production of the above results? In the first, the snuff, and in the second, the water, is the excitor; the impression thus created, the *in-going* nerve brings to the spinal centre, and this second, receiving it, sends it by the *out-going* nerve to the intercostals, which are powerfully excited to contract, and thus made to press upon the lungs so as to produce the act of sneezing in the first case, and that of coughing in the second.

If we trace the act of deglutition, we shall find it accomplished by similar stages of progress. A feather used to tickle the fauces, will, by a like process of action as above, excite the diaphragm to contract — press upon the stomach — and thus produce vomiting. It is the upper portion of the fauces which, when irritated by the application of the feather, gives rise to the act which results in vomiting. If it pass into the pharynx, it may be swallowed, an event which has occurred.



In thus allotting to each portion of the nervous system separate and independent functions, we see a wise and benevolent arrangement of nature for providing for our well-being and existence. For example : while the process of digestion is going on, the animal, in the meantime, requires his brain for other purposes—to keep up his relaxation to external nature, and direct his varied movements in providing for his further wants, etc.

In reference to respiration and its dependence on excito-motor influence, let us reflect on what takes place not unfrequently in early infancy. A child is born but has not breathed, the act of respiration has not yet been established ; and if not begun soon the child will die, *still-born*, as it is called. In order to bring about the first step, let us imitate nature, and commence by applying an excitant. In ordinary cases, the contact of the external air with the surface of the thorax, is sufficient for this purpose ; in cases, however, where that fails, let us apply a still more energetic excitant. And this is what is usually done when a little cold water is sprinkled, or if necessary, dashed on the chest of the infant, with frequently the effect of exciting inspiration. This is to take advantage of the mode which nature has established to accomplish the same end—the establishment of excito-motor action—for the performance of one of the most important functions in the animal economy, without which, the infant after its birth, could not continue to exist for many minutes. It is, by understanding the operations of nature aright, that the physician can be successful in such cases in assisting nature, by bringing her own appliances to bear with the necessary energy in the accomplishment of her purposes. Following out these suggestions, he had proposed and adopted the plan, and others had followed the example, with much advantage—of placing the still-born infant, where there was difficulty experienced in exciting the respiratory process, in alternate baths of hot and cold water, which plan served to intensify excitation on the surface. If warm water be first employed, the heated surface, when plunged into cold water, will receive a greater impression from the shock, than if this had not been premised ; and the very same thing is the case if the cold be first employed and rapidly followed by the hot. By these alternate steps to rouse action, he had the satisfaction of knowing not a few had been resuscitated, who, without the employment of these means, would not now, in all human probability, be living. To be instrumental in saving even one such is ever a pleasing consolation, which will follow one throughout life. A case which occurred to himself, he next alluded to, as bearing upon the point, and his object in relating it



was, he said, if possible, to impress upon their attention the importance of adopting every means.

He had mentioned that in the excitation of the first act of inspiration, the air applied to the surface is the excitant which serves the purpose in ordinary cases of exciting the in-going nerve, and ultimately through it the first act of respiration is brought about. But what, it may be asked, produces the second, and keeps the function, once established, in continuous operation? He would answer: the carbonic acid, which the air inhaled and brought into contact with the blood in the tissues of the lungs, serves to form and to eliminate in its way out, irritates the pneumogastric nerve, and becomes the excitant of the second act, and this goes on through life. In this manner, the air is continued to be introduced, and the blood vivified. These functions which have now been described, have a relation to animal life. The next function in order, is that which relates to the perpetuation of the species. This cannot now, for want of time, be gone into, and must be dismissed with the remark, that the varied operations by which this is effected, are of the same nature as above — are *excito-motor*.

The cerebrum relates to all without — to all constituting intellectuality, while the ganglionic system relates to all within. As already remarked regarding food, before it is within, all the operations connected with it are under the control of the former, but no sooner is it within, than it is under the control of the latter, where it remains till the whole process is completed. With regard to ingestion and egestion, we have orifices which receive their excitement to action from different portions of the spinal system; those which are situated at the entrance of the food and air passages, are under the control of the medulla oblongata, which regulates these — the upper; while the lower sphincters, connected with the acts of defecation and urination, will be found to be dependent on a portion still lower down. There is an enlargement at this point of the spinal system, and it is this spot which is the most excitable. It is this region which relates to all the parts of the body lower down. By studying these matters aright, we are in possession of important physiological truths; and these facts thus obtained, will be found to be valuable in pathology and practice.

If you lay bare the brain of an animal and irritate this, and note the results as they manifest themselves, you will thus become possessed of facts which may be of the greatest use in explaining various diseases connected with this region. In the same way, if you lay bare the spinal marrow in its course, and let the result be well noted, this will again be useful in the

same way. Let each one so informed ask himself, now, what phenomena he would expect to be present when there is disease of the brain; and what, when there is disease of the spinal system? In the first of them he would have morbid phenomena of functions of the brain; in the second, those produced by irritation of the spinal marrow. The different effects seen in the first, were then enumerated; these, he said, were all intellectual. The convulsions in the second, were the same in character as those produced by the experiments on the spinal column. When these respective effects are witnessed, as the result of disease, the physician can say with great certainty, if he has carefully studied this branch of his profession, in what part the disease is situated, and the degree of danger which may be connected therewith.

In cases where the disease is lower down than the second portion which controls the action of the thorax, and gives, as we have seen, rise to convulsions, another diagnostic to guide us will be the absence of reflex action. If the disease exists below the lower enlargement formerly alluded to, affecting the cauda equina, as no nodule of spinal centre remains, so all reflex action will be absent. Disease in the second division, may give rise to incontinence of urin, etc.; while in disease of the third, there may be construction of these orifices, or otherwise.

A careful study of these separate effects, enable us to say in this way, what part of the nervous system is affected; and this knowledge carried to the sick-room, will make clear to our comprehension all this class of diseases; and do for the diseases of the nervous system what the introduction of the use of the stethoscope has done for diseases of the chest. Have these facts perfectly in mind, therefore, ready for use, whenever occasion requires.

Next to the study of these diseases and their seats, is the study of what relates to their therapeutics. If there be a physiological action, there must be a pathological condition, and there must be a therapeutic also. Strychnine, administered to an animal, affects all parts with titanic spasms. If you divide the animal into three parts, all portions will be found affected with these spasms — the tail no less than the head. It therefore, affects all the spinal system, and it does not affect the cerebral or the ganglionic. It will be found also, to affect some of the orifices. An overdose, especially acts on the medulla-oblongata, and in this way, rapidly destroys life. Cantharides, again, is different; its physiological effects are more particularly confined to the bladder; and ergot, in the same way, acts on the

uterus. In the selection of any of these for use, consider well what is their action on any special point, and then select that which is most appropriate to the affection. I need not say that ergot is the one to be selected, when the object is to excite the expulsive action of the uterus. If you want to study Obstetrics, make yourself familiar with all the phenomena of labor; these are dependant on the spinal marrow, and in this, and every other branch of your profession, a study of the spinal system will be of great service in enabling you to be useful. What I have said regarding the mode of treating the still-born infant, is illustrative of this, and will amply reward you for all the labor which may be bestowed in acquiring this knowledge.



## Lectures on the Nervous System.

By MARSHALL HALL, M. D., of London.

### No. II.

DELIVERED IN THE UNIVERSITY OF LOUISIANA,  
MONDAY, JANUARY 16th, 1854.

*Gentlemen:* In my lecture of Saturday last, I endeavored to explain and to demonstrate by experiments, the proper division of the Nervous System into three distinct parts; first, the Cerebral; secondly, the Ganglionic; and thirdly, the Spinal or Excito-motor. Our attention to-day will be directed to the last—the SPINAL SYSTEM. This is denominated excito-motor, because its manifestations are invariably the result of some excitant. It is the spinal system which governs all the phenomena of ingesta and egesta; and whenever there is disorder of this system, there is also disorder of those functions.

In morbid conditions of the brain, the attendant phenomena are restricted to the senses. These are intolerance of light, intolerance of sound, headache, morbid acuteness of taste, smell, etc., or the opposite states; but so long as the spinal system remains unaffected, there is never anything resembling spasm. The manner in which cerebral disease is propagated to the spine, seems to be this. In softening of the brain, for example, its volume is actually enlarged; but as the cranial cavity is unyielding, the influence of compression can be propagated in but one direction—that of the medulla oblongata. Pressure in this direction excites spinal disorder,

which is manifested by spasmodic actions in the organs dependent upon this portion of the nervous system. A case of acute hydrocephalus may continue for days, and be characterized only by intolerance of light or noise, disturbed sleep, occasional crying out, as from a frightful dream; but there shall be no spasmodic action manifest in any organ or muscle. I was applied to for advice, in the case of a child, (which exhibited no marked symptom of disease, except feebleness and drooping, and a daily sudden fit of vomiting,) as to the propriety of removing it to the sea-side for the benefit of change of air. I decided against removal. The child died, within ten days, of unequivocal hydrocephalus. In such cases, the supervision of strabismus is significant of danger.

Spasmodic actions can, by no possibility, have existence except in virtue of affection of the spinal system. Manifestly then, the supervision of spasmodic phenomena, in any case of cerebral disease, is diagnostic of spinal affection. If these be exhibited in the arm, the locality pointed at is that part of the spinal centre giving origin to the brachial plexus; if in any of the orifices or sphincters, it is in what I have called the inferior medulla oblongata; if in the lower limbs, it marks a disorder of the corresponding portion of the spinal centre. You will here perceive three facts, which are of directly practical importance, because diagnostic of affections of special localities. I need not enlarge upon the precious value of anything that thus leads us to the door of truth.

But I have an important question to ask. It properly belongs to a later stage of our present investigation; but, on account of its importance, I will venture to introduce it here.

How does spinal disease affect the brain?

I will not attempt to answer now, but will request you to endeavor to solve the problem for yourselves. Meanwhile, I proceed to consider the following facts:

Epilepsy, as you all perfectly know, begins with spasmodic phenomena: they are, indeed, its characteristics; but the condition which is the sequel of epilepsy is coma. Now, all spasmodic conditions are identical, whether they be those of infantile convulsions, or of epilepsy; for they all depend upon the spinal system. If I insert a probe into the substance of the spinal marrow of this alligator, just behind the base of the skull, the result, if the animal (which has already suffered from a number of experiments,) be sufficiently lively for any impression, will be excessive motion of the limbs and the tail. This is the type of centric or direct epilepsy; of that



kind of epilepsy which is caused by a tumor within the brain, by too much or too little blood, by depravation of the blood, (as in diabetes,) or which is the sequel of apoplexy; all which originate in the nervous centres. On the other hand, the motions which were exhibited on Saturday, and which I now re-produce by pressing the foot or the tail, are the type of eccentric or reflex (in-going and out-going) epilepsy. Of this kind, are the epileptic convulsions caused by dentition, by indigestible substances in the stomach, by constipation, by worms, and by uterin irritation, whether from catámenia, or gestation, or the puerperal state. The direct or centric form of epilepsy, is by far the most difficult of cure.

All the orifices — the exits and entrances of the body, depend for their nervous influence on the superior and inferior portions of the spinal system respectively.

The first phenomena of epilepsy may be the slightest appreciable muscular contractions — slight contraction of a finger, slight strabismus, twitching of an eyelid, etc., — but by far the most important is the *torti collis*. This may amount, indeed, only to mere rigidity of some of the muscles of the neck, but its importance is none the less great. It will be succeeded by flushing of the face, fullness of all the tissues about the neck, (inso-much as to increase its circumference, in one case which I measured, to the extent of an inch and a quarter,) compression of the veins preventing the return of the blood, and thus engorging the brain to a dangerous degree; next by laryngismus and the peculiar cry, characteristic of convulsions, which is sometimes loud and terrific, but oftener a constrained moan or whine, which, once heard, is not easily mistaken or forgotten; the whole of the phenomena being those of excessive pressure of the brain, and the final result not being perfect relief, but bewilderment or stupor, or even mania. Indeed, this a common origin and cause of mania. A fit of epilepsy occurs in the night; nothing is known of it, for there was no witness; but the person is observed to be strange, or dull, or absent in manner. I knew a case in which the gentleman, a bachelor, exhibited a strangeness of manner which was remarked, every few weeks, by his servants, who were in the habit of saying, "Master is odd to-day:" but no one suspected the cause. After his marriage, his wife soon discovered that he was the victim of epileptic convulsions in his sleep. In any of these unaccountable cases, where a person exhibits an altered manner, a strange dullness, a disposition to fall into revery, or unreasonable sadness, it becomes a matter of serious concern to assure yourself upon this point, whether he is not the

victim of epilepsy in his sleep. By all means examine the tongue, and ascertain whether it be not sore from biting it, or from thrusting it against the teeth; examine his pillow, to see if there be no marks of blood or froth upon it. Such epilepsy, often leads directly to the particular form of homicidal mania. A poor girl killed a child which she was nursing, and she was tried for it on the charge of murder. In my investigations of the case, I discovered that she had been habitually subject to epileptic convulsions; and I have the happiness of believing that my testimony on this point, rescued from an unmerited and ignominious death, a poor victim of disease. Indeed, I have no doubt that many persons, from this very cause, have suffered as felons, who were perfectly innocent of moral guilt. The gentleman who was observed to be "odd," every five or six weeks, was sent to an asylum. I predicted that he would not remain long there; and in fact, he did remain but six weeks; but I am ignorant of his fate.

I believe most cases of true mania, originate in hidden epilepsy; and I consider epilepsy, mania, and dementia, to be different phases of one and the same disease. The sequel of an epileptic convulsion, may be raving mania. This may be relieved, and the patient recover; but the sequel is dementia, more or less complete. This is not all: there may be partial or spasmo-paralysis. One of my own students, after an attack of epilepsy, had spasm-paralysis of one arm, which obliged him to carry it in a sling. He endeavored to continue his studies; but was obliged, after a time, to abandon the profession, in consequence of the fitful injury to the intellect. The particular state of spasm-paralysis, depends upon disease exclusively spinal; where as hemiplegia has its origin solely in the cerebral hemisphere of the side opposite to that which is affected.

In every case of mania, it is important to inquire whether epilepsy has not preceded it; for epilepsy may produce, upon the brain itself, all the phenomena of original cerebral disorder. You have stertor, or paralytic laryngismus, the same as in apoplexy; you have pressure on the pneumogastric nerve — more especially the bronchial branches of it — resulting in the outpouring of excessive quantities of mucus, and all the phenomena of mucous rhoncus; you have pressure also upon the branch which is bestowed upon the stomach, causing impaired digestion, with flatus and distension, as in apoplexy. The tracheal rhoncus of epilepsy is identical with that of apoplexy; and the paralytic state of the pneumogastric nerve succeeds the coma of the one disease, with the same certainty that it does that of the other.

Gentlemen, I wish to inquire how much of all these phenomena depend upon the laryngismus? No question can have a higher interest than this — how much of all the phenomena peculiar to epilepsy and apoplexy are dependent upon the simple mechanical obstruction, caused by the laryngismus? It is eminently a practical question; for the life of the patient cannot be saved while this condition is unrelieved; and, to accomplish that relief, I know of but one efficient means — the operation of tracheotomy. By tracheotomy alone, may we relieve the laryngismus, which is the most frequent cause of fatal cerebral engorgement, and thus save the life and the mind.

In my former lecture I said that strychnia in the vegetable kingdom corresponds with epilepsy in the animal kingdom. It is the therapeutical agent by which we may artificially induce a condition which is the perfect counterpart of the pathological state in epilepsy. Its influence upon the pharynx and larynx is not distinguishable from that induced by spasmodic disease.

The following are the results of experiments upon four dogs: To the first I gave a dose of one-sixth of a grain of strychnia. After a proper time, finding no sensible effects produced, I repeated the same dose. Very shortly its influence was manifested in the most exquisite degree. I could not touch the dog — not the tip of an ear or even the extremity of a hair, without instantly exciting severe convulsions. Even the approach of my hand to his sight, without contact, was sufficient to produce them. The dog died in consequence of the recurrence of convulsions thus excited; and it really becomes a matter of serious interest to inquire how far the symptoms are aggravated, and even the fatal result hastened in tetanus and hydrophobia, by officious kindness, by visiting, by noise, by touching, even in feeling the pulse. Personal apprehension certainly has the largest share in the renewal of the convulsions of hydrophobia, by the act of pouring out water within the apartment of the patient; for nothing of the kind happens when the cause of fear (the phobos) is removed by pouring the water out of doors or in an adjoining apartment.

To a second dog, as nearly like the first as I could procure, I administered, in like manner, a first and second dose of one-sixth of a grain each. I conveyed him to a soft bed, in a secluded room, to which no person had access but myself, and guarded him constantly from every possible source of disturbance. The following day the dog was perfectly well.

To a third dog I administered three doses of one-sixth of a grain of

strychnia, but he died suddenly, in a strong convulsion, with laryngismus, from the direct effects of an over dose.

To a fourth dog I gave the same three doses, and as soon as convulsions appeared I performed upon him the operation of tracheotomy, and used all those precautions to ensure quiet, that were employed in the case of the second experiment. The convulsions ceased, and the dog was living the next day. He, however, refused all food or drink, and finally died of exhaustion.

I believe hydrophobia may be *stopped* by tracheotomy! All the victims of this disease have hitherto died of laryngismus; if, then, the trachea were opened below the seat of the spasmodic stricture, whatever might be the result, the patient could not die from that cause. We know not whether he might not be destroyed by the excitement alone; and perhaps the direct influence of the poison may be such as to destroy life, independently of the mere convulsive phenomena; but, I repeat, he could not die by the laryngismus, and by tracheotomy only can we ever ascertain to what extent the death is really due to the exhausting excitement, or to the virulence of the poison. In this connection I will mention that the celebrated Dr. Physic, of Philadelphia, on witnessing a case of hydrophobia, observed that "the immediate cause of death was the laryngismus," and he actually suggested this very operation as the means of obviating it.

The operation of tracheotomy is really a very simple one. I am no surgeon, and am very far from a disposition to boast, in the presence of gentlemen so distinguished in that branch as those now before me; yet, I am confident I could perform the operation blindfolded. I would make an incision through the integuments with a knife, and then open the trachea with an instrument like a pair of pointed scissors, by thrusting them, closed, into it, and then expanding them horizontally, or right and left—until the opening was sufficient. The opening is to be preserved and protected, by inserting the "wire-cage." [Dr. Hall, in a conversation subsequent to the lecture, explained to the writer that the wire-cage is formed of a small silver wire, about the size known as "No. 56," or one-twenty-fourth of an inch in diameter, and twelve inches in length, which is bent into eight equal parallel parts, each an inch and a half in length, so as to resemble four slender staples all united by their legs. When completed, they should be so arranged as to define the rude outline of a cylinder three-quarters of an inch in diameter. By grasping and compressing all four of the loops or staples at one end, they are readily introduced a little way into



the opening in the trachea, and by their tendency to expand, they keep the opening patent, and are restrained from escaping. Their escape is perfectly prevented by bending slightly outwards each of the four tips before their introduction. N. B. B.] The presence of the "wire-cage" in the opening of the trachea, is not a source of much annoyance. I once had a patient who wore it constantly, without inconvenience, for a year and a half. The tracheal membrane is not so liable to inflame as the laryngeal; and this, among many others, is a good reason for giving to tracheotomy the preference over laryngotomy.

The subject which I have thus endeavored to bring before you is one which has occupied my attention since 1831, and when generalizations are so simple and so clear, and at the same time, susceptible of so much practical application, as a precious means of diagnosis, in a class of diseases confessedly obscure, I trust to your indulgence in thus urging upon you my own convictions of their importance.

Now then, to return to the question which I asked in the early part of this lecture, "how can a disease of the spine affect the brain?" I answer, I can conceive of no possible way except by affecting the muscles of the neck and the larynx.

This, gentlemen, concludes the subject which I wished to bring before you. I thank you for the courtesy you have shown me in affording this opportunity, and for the flattering attention with which you have listened to what I have so poorly said.

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## Editorial and Miscellaneous.

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"NOTES ON THE EPIDEMIC YELLOW FEVER OF 1854; By P. M. KOLLOCK, M. D., *Professor of Obstetrics in the Savannah Medical College.*"

In the late August number of the *Southern Medical and Surgical Journal* we find a very interesting article from the pen of Prof. Kollock, on the subject of the great epidemic of yellow fever which desolated Savannah last summer. The perusal of this article has afforded us much pleasure, as we are deeply interested in all that relates to yellow fever, though we cannot refrain from making a few critical remarks, which we hope Dr. K. will receive in the same kind spirit in which they are offered. Criticism clothed in the garb of charity is a boon to science, and it shall be our aim never to strip her of the vesture.

We would call attention then, to D. Kollock's recorded experience with quinine, as a remedial agent in yellow fever. He recites cases wherein he exhibited it, detailing the time when, and the doses in which he gave it, and sums up his experience with the following cursory remarks:

"In the cases which show the yellow fever marks, quinine is certainly injurious, inducing irritability of stomach and hastening the advent of black vomit."

Now the sum and substance of that which we wish to say is, that Dr. K. has not administered quinine *secundum artem* in yellow fever, and, consequently his conclusions fall to the ground. To his first case Dr. Kollock gave six grains of quinine, on the 11th August, in one dose; August 12th, five grains in the morning and five at one o'clock, P. M.; August 13th, two and a half grains at half past seven, A. M. To the second case reported he gave, August 15th, six grains of quinine; August 16th, five grains. Both these cases recovered.

To the third case quinine was administered, but the quantity is not mentioned. The patient died. To the fourth case he gave six grains of quinine on August 15th; 16th, five grains every three or four hours; August 18th, ten grains with one-fourth of a grain of opium — this was given in a remission, and the patient grew drowsy and ultimately died. To the fifth case he gave five grains of quinine, August 17th, which seems to have been repeated, but the intervals are not stated. Patient was considered well, but on the 20th fever recurred, and then, says Dr. K., "I accordingly bled him pretty freely from the arm, gave him blue mass and quinine as long as his stomach would bear it, which at length became irritated, rendering it necessary to apply a blister, etc." Patient died September 5th. "When called to a patient with considerable rigor, a full and corded pulse, I have generally commenced the treatment with *vs.*, followed by calomel and sulph. quinine aa. ten grains, or ten and five — in three hours after, *ol. ricin.* ℥ ss. or ℥i; iced gum water, toast water, or acidulated flaxseed infusion. In some of the first cases I saw, I continued calomel and quinine in small doses, gr. two and five, repeated at intervals."

Finally, Dr. Kollock winds up his experience with the following recital: "August 25th, I have under treatment an Irish woman who had been ill for five days, and had been actively purged with calomel and jalap. I was called to her in consequence of her having thrown up a small quantity of dark brown specks, resembling fragments of insects' wings. I blistered her stomach and extremities, and gave her iced gum water. After blistering, although the stomach continued irritable, I determined to test the powers of quinine, as asserted by some,  $\mathfrak{Dj}$ . of the sulphate was administered and repeated in four or five hours. After the second dose, the vomiting ceased in a great measure; but as some irritability remained, effervescing cit-soda, and ten grs. more of quinine were given. This last dose was succeeded by great uneasiness, vomiting, deafness, and flushed face. The pulse became very much lowered, tongue remained clean and moist. Ice was applied to the head; stimulating enema and soda continued. She became comatose and died in a few hours. The cerebral symptoms I attributed to the quinine."

Before we go farther, we would be distinctly understood as not offering ourselves

as the particular champion of quinine, but rather of the whole materia medica. We do not stand here to nurse a pet theory of our own as to whether quinine is the best remedy for yellow fever, but to war against the positive abuse of a remedy being taken as evidence against it when brought before the judgment seat. Dr. K. says he has administered quinine in yellow fever, and decidedly pronounces against it. We say that he has not only not used it properly, but has absolutely *abused* it. To his first five cases named, when he did give the remedy early in the disease, he gave it in totally inadequate doses — such doses as we have never seen advocated by the friends of quinine. Again, to his first and fourth cases he gave the remedy even on the third day of his attendance, (in the latter case, in a double dose,) which is totally at variance, not only with the indications in yellow fever, but with the plan laid down by the friends of quinine — they always urging the importance of administering the remedy *only* in the commencement of the disease. Again, Dr. Kollock *bled* his patients in the beginning — a practice deprecated by the advocates of quinine, except under extraordinary circumstances. Again, in a case (No. 5) in which fever recurred on the fourth day, Dr. Kollock not only bled the patient “pretty freely from the arm,” but administered “quinine and blue mass as long as the stomach would bear it” — a system of practice advocated by no friend of quinine. Finally, Dr. Kollock gives both quinine and his patient the *coup de grace* by administering the former in *twenty grain doses* to an “Irish woman” who had been ill for five days, and had been purged freely with calomel and jalap! In fact, he gave it to the woman (fifty grains in all) after black vomit had appeared, threw her into a comatose condition, in which she died, and complacently calls this testing “the powers of quinine as asserted by some.”

If we had room in our Journal we would like to cite authority *in extenso* on the subject of the “quinine practice” in yellow fever; we must content ourselves, however, with calling Dr. Kollock’s attention to the little work entitled “*Some Account of the last Yellow Fever Epidemic of British Guiana*; by Daniel Blair, M. D., Surgeon General of British Guiana.” In this work he will find laid down the true principles of the quinine practice. Dr. Blair gives rather more calomel than is given here, but he thoroughly understands how to exhibit quinine.

We must close by remarking that the advocates of quinine here, give the remedy *only* in the beginning of the disease — that is, within the first twenty-four hours; they always precede the quinine with injections or oil; some combine calomel or opium with quinine; they give quinine in *sedative* doses — fifteen to thirty grains, according to circumstances; they apply cups, mustard, etc., etc., to relieve heat, pain or congestion; they never give quinine when there is great irritability of stomach; and, finally, they regard quinine as *poison* in the black vomit stage of the disease.

Had the paper of Prof. Kollock emanated from any other than a Professor in one of our Medical schools, we might not have noticed it at all; but, we must protest against our young men being taught to discard quinine from the list of remedies in yellow fever on such experience and for such reasons as have been detailed.

## CATHETERIZATION OF THE TRACHEA, ETC.

*Turpe est viro, id, in quo quotidie versatur ignorare*

Although long convinced of the utter futility of the much vaunted operations of Dr. Horace Green, of New York city, we have hitherto remained silent on the subject, looking with unwavering confidence to the speedy advent of such an unequivocal and impartial expose of the whole matter by his own New York brethren as would be most satisfactory to the whole profession. Notwithstanding a greater delay than we had anticipated, the time has arrived; the enlightened medical men of New York began to feel the importance of settling definitely this widely vexed question, and in December last the "New York Academy of Medicine" appointed a committee of seven of its members to investigate the matter. The report of this committee has at length been presented to the Academy. The gentlemen composing the committee are said by the *New York Medical Times* to have been chosen by Dr. Green himself, consequently we have no reason to expect objections on his part, either on account of ignorance or personal animosity. Six of the committee (Drs. Parker, Wood, Stevens, Anderson, Metcalf and Stone,) make a "majority report," which is, as we conceive, totally adverse to all Dr. Green's pretensions, and Dr. Barker, of whom the *New York Medical Times* says— "Dr. Barker's interests are identified with those of Dr. Green in this matter, and, therefore, due allowance must be made for his minority report"—made a minority report in support of Dr. Green. In reading the account of this latter report, we are forcibly reminded of the drowning man catching at a straw; strange as it may seem, Dr. Barker at one time so far forgot himself as absolutely to offer to a body of scientific gentlemen, as evidence for the support of his friend, Dr. Green, the certificate of a parcel of *women*, whom Dr. Green had not cured, but had under treatment! 'Tis said that straws show which way the wind blows. We fear this is a "puff" of unwholesome quality.

For some years past Dr. Green has been burning out the trachea (?) of his patients to an extent peculiar to himself; as is always the case, he has had some proselytes who have ever been ready to echo his declarations, for

"'Tis thus the spirit of a single mind  
Makes that of multitudes take one direction,  
As roll the waters to the breathing wind,  
Or roams the herd beneath the bull's protection."

The mass of the profession, however, have rejected both his theories and his practice; some not believing in the practicability of the operations, others denying the utility of the same, even if practicable. Although Dr. Green has been pretty freely assailed in various quarters, still many of his brethren have kept hands off; but there is a time when forbearance ceases to be a virtue. We listened in silence to tales of burnt-out trachea, and even to right and left bronchi catheterized at will; but when Dr. G. proclaimed that he had, in direct opposition to the simple but unerring laws of hydrostatics, injected a dram of a solution of nitrate of silver into a cavity in the lung, he not only took the fatal leap which is ever, sooner or later, the lot of the wild enthusiast, but he offered one of those naked insults



to the common sense of his brethren which could not be too promptly repelled.

For two years past Dr. Green has been in the daily habit of introducing instruments (?) into the trachea; he would have us believe that he is the expert, the Magnus Apollo in this novel (?) art; yet Mary Norton, in presence of the committee, vomited a portion of the contents of her stomach through the tube which Dr. Green himself has introduced, and which he has assured the committee was then in the trachea—the avowal of Mary Norton to the contrary notwithstanding. If this is the degree of certainty arrived at by Dr. Green, save us from his operations! What would be said of the medical man who, if called on to cleanse the stomach of poison, should pump water into the lungs of the patient and drown him? And yet, with all due deference for Dr. Green's skill, we think he is the very man to commit the blunder. If he could not be certain of the whereabouts of his instrument in the case of an individual on whose trachea he had been accustomed to operate, and who insisted that the instrument was in the œsophagus, we maintain that he would be as apt to drown a man with a stomach pump as to wash out his stomach. This is not exaggeration; it is one of those plain, homespun inferences which arises in the mind of any honest searcher after truth. But we pass on to another point of evidence.

In eighteen cases where the committee endeavored to introduce the sponge probang into the trachea, they failed eighteen times—or one hundred per cent. The N. Y. *Medical Times* says these failures were at the hands of Dr. G. himself. With a small curved tube, such as used by Dr. Green himself, there was ninety-two per cent of failures; and with a tube curved “to a form corresponding to a circle six inches in diameter,” there was thirty-eight per cent. of failures. So that it is entirely a matter of accident when the sponge probang enters the larynx; it is next thing to accident when Dr. Green's own tube enters; and in thirty-eight cases out of every hundred the subjects of the injection through the largely curved tube must be content to take the solution into their stomachs. Yet, most lucky dogs are these thirty-eight; for the committee report the speedy death of the only subject into whose lungs they introduced the solution.

We have not room in our journal for as extended remarks on this important subject as we deem necessary; we should have been pleased to call the attention of our young practitioners more closely to all the points; it is this portion of the profession who are more apt to be caught by books baited with highly flavored novelties. This is a progressive age, but it is also the age of humbug. All is not gold that glitters, nor is the dictum which emanates from a medical school any more truth for its origin, than Worcestershire sauce the genuine article for having the label on the bottle.

We have always been, and always expect to be the advocate of specialities in medicine; we maintain the ground that any mind of medium capacity, can master one subject, while few can master all; and consequently, both the profession and their patrons would be benefited by the system. Moreover, a devotion to specialities would better enable us to contend against the hoards of quacks who surround

us. However, we will always be amongst the first to proclaim against the *prostitution* of specialism. If a member of the profession be a good oculist, aurist, surgeon or obstetrician, we will most cheerfully acknowledge the same; but we will ever discard all pretensions not based on reason and sound philosophy. The moment a man aspires to a reputation for performing impossibilities, then is our pen against him—not as a man, for as such we know him not, but as a rebel to science.

This report of the New York Academy of Medicine is one of the severest rebukes ever inflicted on so prominent a member of our profession, though the known character of the six gentlemen who presented the same is sufficient earnest of its justice. While we criticise we indulge no feeling of exultation, for

“The truly brave are soft of heart and eyes,  
And feel for what their duty bids them do.”

Being always proud to acknowledge amongst our ranks intellects such as all most truly accord Dr. Green, we feel but too sorry that he should have thus cruelly ridden a hobby so little able to carry him.

**NECROLOGY.**—Died, of yellow fever, on the 5th of August, Mr. Thomas M. Neal, aged 22 years, a native of Alexandria, La., and a resident student of medicine in the New Orleans Charity Hospital.

He was a gentleman of high intelligence, even in his youthfulness, and prosecuted his studies with zeal and assiduity. In his relations with his fellows, he was kind, generous and affable, and towards his seniors, eminently polite; so that he gained the friendship and regard of all with whom he became acquainted.

Death has cut him down in the half-cultivated field of his labors, expectations and promise, and we mourn his untimely fall with a deep sense of his worth, and of the loss which his widowed mother and numerous friends have sustained.

**NECROLOGY.**—Died, on the 27th of August, 1855, of yellow fever, P. O. Tête, aged 21 years, a native of Assumption Parish, La., and for the last nine months a resident student of the Charity Hospital.

It is with feelings of the most poignant regret that we announce the death of this estimable young man, who, by his industry, devotion to his profession, and modesty, won the love and friendship of all who knew him. He was a most diligent student, and promised by his exertions, to make himself useful in the profession which he had chosen.

By this unlooked for and sad event, has the heart of a fond and doating mother, loving and affectionate brothers and sisters been deluged in the most poignant affliction.

Away from the home of his youth, far from the aged mother who idolized him, it must still be a source of gratification to that mother to know that his every want was satisfied, his every wish fulfilled.

AGASSIZ—*Contributions to the Natural History of the United States.*—We are in receipt of the prospectus of this great work. Professor Agassiz proposes to issue annually for ten years, a quarto volume of three hundred pages, each volume containing about twenty plates, executed in the very highest style of the art. He says, “Without entering into a detailed account of the contents of this work, it may be sufficient here to state, that it will contain the results of my embryological investigations, embracing about sixty manographs, from all the classes of animals, especially selected among those least known as characteristic of this continent; also, descriptions of a great number of new genera and species, accompanied with accurate figures and such anatomical details as may contribute to illustrate their natural affinities, and their internal structure.”

The name of Agassiz attached to the prospectus of such a work, is recommendation sufficient to ensure success in the undertaking. The price of the work, (\$12 on delivery of each volume,) although considerable in the aggregate, is such as comes within the means of most of those who entertain a real desire to advance the sciences by their individual patronage. All who desire to subscribe to the work, will address Messrs. Little, Brown & Co., of Boston, or Prof. Agassiz, at Cambridge, Mass.

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MORTALITY OF BOSTON.—For the week, ending July 21st, ult., the total mortality in the city was 105—against a total of 59 for the week, ending July 14th. This is an increase of nearly 100 per cent. Twenty-three of the deaths were from cholera infantum, and thirteen from consumption; ten from “infantile diseases,” (very indefinite,) and seven from teething. Altogether, there were sixty-one deaths among children under five years, or considerably over half the entire mortality.

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“THE JOURNAL OF MEDICAL REFORM: *For the People and the Profession.*”—We are in receipt of the August number of this delectable sheet. To have sent it to us at all, is quite indicative of the vulgar audacity of the order whence it emanates; but to ask us to exchange, is a specimen of that unscrupulous impudence, which is the exclusive characteristic of the grovelling miscreants who edit all such iniquitous prints. We send the editor this number of our Journal, that he may learn therefrom that our exchange list affords no place for quack advertisements.

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#### HEALTH OF OUR CITY—AUGUST 25TH, 1855.

While we look with sorrow on the vast amount of work which death has done in New Orleans since our last issue, a concomitant feeling of joy pervades us on reflecting that we are once more emerging from this terrible state of existence to which we have so long been subjected, and we are led to hope more fervently than ever, that far better things are in store for our ill-fated city. Surely, there is great strength in a people who can stand all that has been inflicted on us in the

past three years, and if they will but apply their own shoulders to the wheel, things must change for the better. Now that we have learned so truly the great proclivity evinced by these terrible diseases to fasten themselves on us, and sweep us off by thousands, is there any reason why we should not make every effort to direct ourselves, as a community, of all those attributes, (so far as they may be reasonably supposed to be known,) which makes us the "loved objects," the special victims of these scourges.

The report of the health of the city is any thing but a flattering one, though the diminution of yellow fever for the past and for the present weeks is well marked. We subjoin below the list of mortality in the city of New Orleans, for the four weeks, ending August 25th, at 6 o'clock, A. M.

LIST OF MORTALITY.

	1st w'k.	2d w'k.	3d w'k.	4th w'k	Total.
Total number of Deaths,.....	275	336	517	471	1599
Males, .....	168	230	355	294	1049
Females, .....	97	98	154	169	518
Sex not stated,.....	10	8	8	8	34
Whites,.....	253	314	497	456	1520
Blacks, .....	8	16	12	10	46
Mulattos, .....	13	6	8	5	32
Color not stated,.....	1	0	0	0	1
Native Americans,.....	26	59	40	45	170
Northern States,.....	6	11	9	10	36
Western States, .....	5	10	6	5	26
Southern States,.....	15	38	25	30	108
Foreigners, .....	188	225	350	317	1080
English,.....	1	14	5	7	27
Irish, .....	78	75	135	136	424
French, .....	14	29	57	51	151
Germans, ... ..	65	79	129	113	386
Place of Birth not stated,.....	61	52	127	109	349
Age not stated,.....	18	27	23	20	88
Under one month old, .....	6	10	10	12	38
From one to five years,.....	29	30	45	39	143
From five to ten years, .....	7	10	7	9	33
From ten to twenty years,.....	27	36	81	78	222
From twenty to thirty years,.....	97	120	202	169	588
From thirty to forty years, .....	48	56	87	92	283
From forty to fifty years, .....	26	28	29	23	106
From fifty to sixty years,.....	12	12	22	19	65
From sixty to seventy years,.....	3	5	8	9	25
From seventy to eighty years,.....	0	2	2	1	5
From eighty to ninety years,.....	2	0	1	0	3
Over ninety years,.....	0	0	0	0	0
Yellow Fever,.....	173	222	394	357	1146
Other Fevers, .....	81	97	95	85	358
Typhoid Fever, .....	3	3	8	5	19
Cholera, .....	8	2	5	3	18
Intemperance,.....	0	1	2	3	6
Accidental,.....	2	4	3	5	14
Still-Born,.....	6	4	8	10	28
Disease not stated,.....	2	3	2	4	11



CHARITY HOSPITAL.

Since our last issue, yellow fever has been existing to an alarming extent, particularly in the Charity Hospital. We can safely say, however, that the disease is now on the decline, and that in our next number, we will be able to announce the disappearance of every vestige of that dreadful scourge.

Since the appearance of yellow fever in this institution, there have died of the disease 895 persons, while 681 have been sent out as cured. There yet remain in the wards quite a number of convalescents, who are to be discharged.

Two surgical operations of importance have been performed in this institution since we last wrote. One of the operations consisted in the ligature of the femoral artery, rendered necessary by an aneurism of that vessel. The second operation was the resection of portions of both maxillary bones, performed on a patient affected with an encephaloid cancer of the face of some years standing. We will give the full history of these two interesting cases in our next number.

CHARITY HOSPITAL REPORT—For the month of August.

Admitted, -	-	-	-	-	-	-	-	-	-	-	1,598
Discharged, -	-	-	-	-	-	-	-	-	-	-	1,058
Died, -	-	-	-	-	-	-	-	-	-	-	593
Births.—Males, 7; females 6.											

patient's face, and removed; and in proportion as the anaesthesia becomes declared, the apparatus is held nearer and longer to the face. The sensibility of the patient is judged of by pinching and interrogating him, and his silence is the signal for commencing the operation. If this lasts a long time, a second, or even a third dose is administered in the same intermittent manner. All the wounded brought to the Hospital from Alma and Inkermann were thus treated, without the least ill effects. M. Mounier believes there is no necessity to carry the inhalation to the extent of procuring the abolition of motion, and still less the sideration of the nervous system. Excitability of the muscular system, rarely observed, was, in place of trying to subdue it by new doses of chloroform, met by the removal of the apparatus, and in a few minutes the patient was found to be in a condition for the commencement of the operation.

"M. Mounier believes that the familiarizing them with the use of chloroform, and teaching them to operate on the dead body, are two benefits that he has conferred on the native pupils of the School of Medicine at Constantinople, being some among those that French Military Medicine will have left in the East."—*Medical Times and Gazette*. (June 16th.) From *Bull. de Thérap.* Tome XLVIII.

**ATHEROMATOUS TUMORS OF SCALP—Evulsion.**—Mr. Erichsen lately made some very useful clinical remarks at University College Hospital previous to an operation of removing a series of sebaceous growths from under the skin of the head, in a young woman who had been some weeks under treatment. The surgical class were particularly struck with the microscopic characters of these tumors and their contents, appearances so nearly allied to those of the disease termed "mol-luscum," or enlarged sebaceous follicles in other parts of the body.

Mr. Erichsen confined his remarks to those found on the head, or in the hairy scalp, varying often, as in the present case, from the size of a pea to that of a walnut, and essentially consisting of a diseased sebaceous follicle, lined inside by tessellated epithelium, and containing ordinary atheromatous matter, so like scraped Parmesan cheese, or "pap" of bread, as the term atheroma itself signifies. Large quantities of cholesterine were also observable, as well as a darkish colored fluid in the oldest ones—a sign, Mr. Erichsen remarked, of disintegration not unfrequently followed by ulceration. This sort of bad fungus ulcer on the head may be readily mistaken, when seen for the first time, for a malignant growth, if care be not taken to inquire into the history of the case, or other "wens" not looked for. The mode of removal of these tumors before ulcerating is very simple. A single incision is made across the wen or cyst, and then, with a strong drag of a forceps, a sort of evulsion is practiced, the entire growth coming out like an almond from its husk or shell. They are found also under the eyelid, and are made worse by any practice but one—namely, passing a small probe through them on the conjunctival surface, and stirring up the contents with the probe dipped in nitric acid. As a matter of minor surgery, a sort of study of "common things," too often neglected in practice, Mr. E. showed how the tumors were removed, by practicing evulsion on five or six in as many minutes.

**MORTALITY OF MEDICAL MEN.**—In a memoir read before the Physico-Medical Society of Wurzburg, by Dr. Eschschricht, are contained some remarks on the variation of the average duration of life in the different professions. From the statistics drawn up in Bavaria, and which coincide with what Caspar had already proved, it appears that among the members of the medical profession the highest rate of mortality exists. Three-fourths of the medical men die before the age of fifty, and ten-elevenths before sixty. Of 1688 medical men in Bavaria in 1852, four only had passed the age of 80; and of these four none had devoted themselves to the practice of medicine exclusively. The united ages of the oldest ten physicians amounted to 792; while the united ages of the oldest ten in each of the other liberal professions presented the following numbers: Roman Catholic priests, 878 years; Professors in the schools, 875 years; Protestant ministers, 865; lawyers, 885 years.—*Association Medical Journal*, April 27th, 1855, from *Bulletin de Therapeutique*.

NEW ORLEANS

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Non-identity of Bilious and Yellow Fevers.

By J. C. NOTT, M. D., of Mobile, Ala.

Dr. E. D. Fenner published some interesting "Remarks on Yellow Fever" in the August number of the *Medical News and Hospital Gazette*, the principal object of which was to show that yellow fever "is only one of the types of endemic fever, common to this region, and not a *separate, distinct and specific* disease." Dr. F. has had ample opportunities for observing the diseases of New Orleans, and his well established reputation entitle his opinions to full respect. Yellow fever, however, is one of those protean monsters which has not only to be seen often, but by many observers and in many places, before its natural history can be fully comprehended. It may, therefore, be useful to compare the facts collected, from time to time, in Mobile and other places, with those of New Orleans, in order that we may aid each other in this important investigation.

Mobile and New Orleans differ very materially in their topography. New Orleans is situated on a mammoth fresh water stream, nearly one hundred miles above its mouth; Mobile, on the contrary, is situated on a bay, at the junction of fresh and salt water. New Orleans stands on a rich alluvial soil, and is surrounded by a swamp; Mobile is built on a porous sandy soil, and is surrounded by pine lands. In New Orleans, yellow fever occurs much oftener than in Mobile, sometimes as early as June, and frequently in July. In Mobile there are periods of exemption from epidemics of six and eight years, and the disease never shows itself earlier than August. In New Orleans, intermittents and remittents commence early in the spring, and continue, apparently without interruption, throughout the season. In Mobile these diseases usually decline by the 1st of July, and this month is always the healthiest one in the year. If we have

no yellow fever in the latter part of the season, we enjoy a remarkable exemption from all, so called, miasmatic fevers, in the city, though they may occur to a limited extent in the vicinity. In New Orleans, Dr. F. tells us, the intermittents increase in number and violence when yellow fever is about to appear. On the contrary in Mobile, July seems to be an interregnum between the two diseases, and no physician in Mobile can ever pretend, one week, or one day before yellow fever appears, to predict its advent. At the moment I am writing (15th August) there is almost an entire exemption from remittents and intermittents; our bills of mortality have thus far presented not a single instance of death from fever, and yet, hearing of yellow fever in New Orleans, Norfolk and other places, our physicians are on the *qui vive* for the first case. In 1853 the great epidemic fell upon us with the suddenness of an avalanche. My friend Dr. Levert and I, ten days before its appearance, while in conversation on this subject, agreed that no town, from the character of the prevailing diseases, ever had less cause to fear yellow fever, and yet this scourge was destroying about two hundred a day in New Orleans, and we confidently expected soon to have our turn; these anticipations were most painfully realized. This disease, too, not only ravaged our city, but extended into the pine hills—at Spring Hill, six miles—Citronelle, thirty-three miles, and Grove Hill, one hundred miles, *where intermittents are almost unknown*, and raged, if possible, with increased violence. This abrupt commencement of yellow fever has been a general characteristic in Mobile.

Writers often speak of yellow fever as a *modification* of our marsh fevers; this idea is a vague one and difficult to seize. Yellow fever, when fully developed, presents such a peculiar type, that some very *distinct modifying cause* must be added to that of marsh fevers, before such a type could appear. This new cause, too, is produced in crowded populations, and not in the country, as are bilious fevers; so true is this that while yellow fever properly belongs to towns, intermittents belong to the country, and always disappear from towns when they become very large. While intermittents are common on the Thames, above and below London, Watson tells us that they are absolutely unknown in the city. So with Philadelphia; though intermittents are common on the Scuykill and Delaware rivers, they have disappeared from the heart of the city just as paving has extended. All this would go to show that marsh and yellow fevers are not identical.



Dr. Fenner advances three principal reasons for believing in the identity of these fevers.

1st. Their prevalence in the same locality and at the same season of the year.

2d. The difficulty of establishing, in many cases of mixed character, well marked diagnostic symptoms.

3d. The fact that intermittents often run into well marked yellow fever, and the latter sometimes terminate in intermittents.

Now, I freely admit the truth of the foregoing propositions, but to my mind they afford no evidence of the identity, or even consanguinity of the diseases. What may be termed *hybridism* in disease no more proves identity of *species* than does hybridism in animals. The mingled type to which Dr. F. alludes, no more establishes the identity of yellow and marsh fevers than the existence of a mule proves the horse and ass to be identical—in fact, it may be received as a well established axiom, that no two atmospheric diseases *can prevail together without blending their types*; the whole history of epidemics proves this. We may instance the group of acute eruptive diseases—that of fevers, of bowel affections, including cholera, etc. *Diagnosis* is one of the great difficulties of our science, even in acute diseases disconnected from morbid poisons. The most accomplished pathologist is often at fault in distinguishing the lesions of the brain, of the chest, of the abdominal viscera, etc., and the difficulty is by no means lessened when we have to deal with that class of diseases which are supposed to arise from morbid causes that often float around us in the air, single or combined.

Whether yellow fever be a disease indigenous to the soil of the United States is a question not yet determined, and probably never will be. Let us, however, assume for a moment that it is an imported disease, arising from a distinct morbid cause, and see what would be the result of its intercourse with our endemic diseases. It will be conceded that typhoid fever is a foreign and not endemic disease of New Orleans, and yet what would be and what has been its influence on the endemics? It is a disease of cold and not of warm weather, and therefore not likely to come much in contact with bilious and intermittent fevers; yet, I presume no one would doubt that if typhoid fever could be turned loose in New Orleans in July, the endemic fevers would put on more or less its livery. Practitioners in the country are constantly telling us through the journals that typhoid fever has not only been mixed up with the fall fevers, but with all

acute inflammatory diseases, as pneumonia, etc., in fact so extensive has been this amalgamation, that many physicians of the interior of the Southern States deny that genuine typhoid fever has been seen at all at the South. On the other hand the fact is familiar that pneumonia is often blended with intermittent fever. Typhoid and intermittent pneumonias are familiar to all.

Again, Asiatic cholera, which is an imported disease, often prevails at the South at the same season and in the same localities with endemic bowel affections, and what physician will pretend to be infallible in drawing the line between diarrhœa, cholera morbus, and Asiatic cholera. That the latter is often grafted on the former, no one will deny, and in the milder cases we are at fault until some *diagnostic* mark of cholera appears. So with intermittent, bilious and yellow fevers, we may well doubt until the characteristic sign appears. However suspicious the case of bilious fever may be, and however violent in grade, if there be *bilious* vomiting after the first forty-eight hours, we can safely say it is *bilious* fever; if there be yellow skin and black vomit, hemorrhages, etc., it is *yellow* fever. In many cases, the poisons are actually blended, and the disease may commence with one type and end with the other.

Another familiar illustration is afforded by acute *eruptive* diseases. Does not every writer on the subject tell us that when small-pox is epidemic, all the eruptive diseases of the same class assume its livery, and the diagnosis in many cases is difficult?

Small-pox, scarlatina and measles are all known to be Asiatic diseases, and to have been imported into America by the white conquerors, yet they have become fully domesticated here, and obey all the laws which characterize them in their native land.\* Small-pox belongs to a different classification, but measles and scarlet fever are closely allied, as are bilious and yellow fevers; and it may be laid down as a law, that diseases, like animals, of *allied* or *proximate species*, will amalgamate and produce *hybrid* offspring. Dr. Rush, half a century ago, made the remark, that so rare was this disease in America, that no physician would be likely to meet scarlet fever more than once in his lifetime; and yet no epidemic is more common in our day. Scarlet fever occurred for the *first time* in South Carolina, where I was then practising, in 1832, and in its genuine form, as described by the old Scotch and English physicians. It made its first

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\*These diseases were supposed to have been brought into Europe by the Crusaders, and were certainly unknown among the aborigines of America. Small-pox was known at least 1000 years before Christ in China.

appearance in Pensacola and some of the West India islands. I moved to Mobile in 1836, and was informed that a severe epidemic of scarlet fever had prevailed the year before in this city. About four years after, scarlet fever and measles occurred simultaneously in Mobile, and *the two diseases became so blended that it was impossible to make a clear diagnosis.* This mingled type or hybrid offspring has existed more or less from that time to the present, whenever either disease is seen. It is a common occurrence in Mobile to see a case of well marked measles end in scarlet fever. First we see catarrh, sneezing, watery red eyes, discharge from the nose, cough, well marked measles rash, and, after a few days, the sore throat and scarlet fever tongue appear, and the case ends in the disquamation of scarlet fever and dropsy. I have no doubt that the same mingled type is seen in New Orleans.

Measles and scarlet fever, as do small-pox and cholera, lie dormant for a time and then rouse up to their work of destruction—all, too, imported diseases. Why these periods of repose and activity we know not. So with yellow fever.

Yellow fever is the disease of Southern cities, and, *par excellence*, of sea ports and towns having water communication with places habitually affected with this disease. Intermittent and bilious fevers, on the contrary, are diseases of the country, and when a town is built on a spot where those endemics prevail, as Philadelphia, Charleston, New Orleans, in proportion as the town spreads, these diseases are driven to the suburbs. New Orleans is a very long, narrow city, extending along the river bank for many miles, and backed, through its whole extent, by extensive swamps. Intermittents and remittents are endemics in this city, which appear *every year without fail*; and though cases are seen every year of sufficient violence to cause death, yet these do not put on the diagnostic symptoms of yellow fever, as do a large portion of the cases of genuine yellow fever which stop far short of death. Now, let us suppose, as usually happens, that the endemic intermittents and remittents are prevailing to a considerable extent in June or July, and that yellow fever is suddenly introduced, what does the history of epidemics teach us would be the inevitable result? Would not intermittents and remittents run into yellow fever (just as diarrhœa would run into cholera)? Would not the epidemic, if severe, swallow up the endemic, and would not intermittents crawl from their lurking places, when the monster had departed? Can any one imagine a different result?

The foregoing remarks give us, to say the least, a strong probability of

the *non-identity* of bilious and yellow fevers. But the story is not half told. If the reader has not gone back to the early history of yellow fever, as detailed in the New York and Philadelphia medical journals, from 1793 to 1823, he has much to learn. Numerous examples are there given where the disease has wandered from the abodes of intermittents, and prevailed in its greatest violence where these diseases are almost unknown. This fact has been again strikingly illustrated in 1853, when the disease visited our pine-barren settlements, to which persons have long resorted with the perfect confidence of security. Spring Hill, Citronelle, and Grove Hill, at the distances of seven, thirty and one hundred miles from Mobile, in the high, healthy pine lands, during that year, were devastated by yellow fever, as were, also, Montgomery and Demopolis, on the rivers, some four hundred miles, by water, in the interior. In short, the disease visited every point within the reach of steamboats and railroads, below latitude thirty-three degrees.

Charleston, South Carolina, affords an instructive illustration of the distinctiveness of bilious and yellow fevers. This city is one of the most salubrious in the United States, and enjoys a remarkable exemption from both of these types of disease. So exempt are the citizens from yellow fever, that it is here known under the appellation of *strangers' fever*; while, on the other hand, the surrounding country is terribly afflicted with bilious fevers, of all grades. Yet, the inhabitants of the country and city dare not exchange places during the prevalence of yellow fever in the latter. If the citizen of Charleston sleeps a night beyond the city limits, he dies with "country fever;" and if the countryman comes to the city, he dies with yellow fever! No city acclimation can protect against the "country fever," and no country acclimation can protect against yellow fever. And still, long residence in the city, or a single attack of yellow fever, in a new comer, is a perfect safe-guard against yellow fever for the future. I hear a great deal about second attacks of yellow fever, but I believe their frequency is greatly exaggerated. Persons may have an attack of bilious fever in New Orleans or Mobile during the prevalence of yellow fever, and it is pronounced at once yellow fever. If the same subject is attacked with yellow fever in a succeeding year, it is at once pronounced a second attack. Twenty-five years observation has afforded me very few examples of well-marked second attacks. Either the first or second attack has been wanting in distinctive character. Many physicians take it for granted that every case, if yellow fever is prevailing as an epidemic, must be of that type.



It has been thus far assumed that yellow fever was an imported disease. If it be assumed, on the contrary, that it is an endemic, the argument is not altered. Like plants, insects and animals, two diseases of the same *genus*, but distinct *species*, may spring from the same soil, and, when co-existing, may obey the law of amalgamation we have laid down, and produce hybrid offspring. When yellow fever occurs in vessels at sea, or in localities where intermittents are unknown, this hybrid type is not seen. When it occurs in a mild form in a place like New Orleans, where marsh fevers are common and actually prevailing, the hybrid character is common. When it occurs as a great epidemic, it is generated so much more rapidly than the cause of the endemics, it soon overwhelms and almost exterminates the latter. Obeying the law of all epidemics, it imprints itself on every disease that comes in its path for a time, and then exterminates. I have, in one of the back numbers of the New Orleans Medical Journal, advocated, as most in accordance with known facts, the animalcular origin of this disease, but will not open that point here.

Dr. Fenner, to prove the identity of intermittent and yellow fevers, adduces the familiar example of intermittents putting on the type of yellow fever and then again assuming the intermittent type. But unless there were distinct antagonism between the two diseases what else could we expect? Each disease is nothing more or less than a case of *poisoning*, and should be reasoned upon as such. Suppose a snake should bite a patient laboring under intermittent fever, or he should swallow a large dose of opium, prusic acid, strychnine, or other poison in dose sufficient to endanger his life for some days, should we be surprised at the reappearance of the intermittent fever, after the subject had rallied from the effects of the intercurrent poison? Certainly not. I have known the same thing to occur with small pox, scarlet fever, and other diseases. There is no reason why yellow fever may not run its course and leave the intermittent poison lurking in the system, ready to resume its course.

So far from the argument of Dr. Fenner, according to my view, being satisfactory, the fact that he adduces is the most conclusive possible against him. If intermittent and yellow fever are attributable to the same morbid cause, would it not be an anomaly without parallel, that the violent form (yellow fever) should attack a subject laboring under the mild form (intermittent) and yet that the latter should reappear and resume its course after the other had disappeared? with the fact before us, too, that one attack of yellow fever protects all grades, great and small, of its own type!

With regard to the *contagiousness* of yellow fever, much might be said, but this is not my purpose here. While I have contended for twenty years that the *germ* of this disease, in whatever form it may exist, is carried occasionally in vessels from one port to another, turned loose and there propagated, on the contrary I have always, previous to the year 1853, stoutly opposed the idea of its transmissibility directly from *one human subject to another*. Many facts during that epidemic aroused strong doubts in my mind, but my opinion is still in suspense. The *animalcular hypothesis*, though but an hypothesis, accords better with its mode of transportation and propagation than any other.

I might also say much on the subject of *quarantine*, but I will close with but a word or two on this. The Gulf of Mexico is the *home* of yellow fever, and I have never known a bad epidemic to occur here, showing itself in several prominent points, as Vera Cruz, Havana and New Orleans, that it did not sweep around the whole coast as far north at least as Pensacola. I should, under these circumstances, think quarantine useless in Mobile and New Orleans. 1st, It greatly obstructs commerce. 2d, It is impracticable on account of the incessant ingress of small craft, night and day, which may reach us in two or three days from yellow fever ports. 3d, Because yellow fever, whether originally imported or not, has become a fixed resident in these cities, and is seen in sporadic form every year.

On the other hand, I would by all means advise our Northern cities to quarantine; they are beyond the yellow fever latitude proper; vessels have to come a long distance to reach them from yellow fever ports, and they have a fair chance of stopping the infected vessels at the quarantine station. From facts in my possession I have little doubt that yellow fever would have attacked New York in 1853, had it not been for the quarantine.

It has not been my object in this hurried sketch to treat any point of the subject as its importance deserves, but simply to draw attention to what I conceive a very important and neglected law, viz: the *genuine* and *specific* character of zymotic diseases and their tendency to hybridism.

Dr. Fenner has been doing good service in the investigation of yellow fever—I hope my suggestions may at least afford him some material for reflection.

## Dr. Hagan's Cases of Abscess.

IBERVILLE, 12th September, 1855.

*Messrs. Editors:*—A brief report of the following cases may prove acceptable to your readers.

CASE I. Delphini, a field hand, aged about 43 years, was taken on the 25th of April with labor pains—her eighteenth pregnancy. I was called about six hours after the pains had commenced. The waters had been discharged previous to my arrival, and on examination, I found the presentation natural.

In consideration of the history of her previous accouchments, having all proved rather tardy, but in other respects, of an uncomplicated nature, and in the present instance no serious symptom presenting, the subject being of robust frame and apparently healthy, I concluded to trust, for the time being, to the efforts of nature, and left my patient with the promise of returning in a few hours, merely advising occasional frictions over the lower portion of the abdomen. On my return delivery had taken place, but, owing to ignorance or neglect on the part of the nurse, in failing to separate the cord from the neck, the child was still-born.

I found the patient suffering somewhat from exhaustion, attended with agitation of the nervous system, all consequent on the loss of a considerable quantity of blood. A grain of sulphate of morphine, however, soon quieted her and afforded her refreshing sleep. Next morning I found her as comfortable as could be expected. She continued to do well until the ninth day, when she was taken with a light chill, followed by fever of a well marked remittent type. This fever lasted six days, after which she improved rapidly for a week, when I was again sent for, and found her with a severe chill, and complaining of a lump, as she expressed it, in the right side of her abdomen. On careful examination I could discover no perceptible difference in the appearance of the two sides, but she exhibited some *uneasiness* on pressure over the right iliac region, though not sufficient, in the absence of other symptoms, to induce me to suspect anything of a serious nature in that locality. I therefore turned my attention to the *chill*, and, suspecting her of having recently indulged her appetite too freely, I ordered pulv. ipecac, grs. xx, and awaited the result. This operated promptly and freely, and relieved her stomach of a quantity of recent ingesta. This relieved her somewhat, but the chill was soon followed by

a high fever, which lasted the ordinary period of an intermittent, succeeded by profuse perspiration. These symptoms continued every other day for about ten days, despite the free administration of quinine and opium, with concomitant treatment. From this time the chills became less severe and of shorter duration, but occurring every day, and sometimes twice a day, followed by fever of a typhoid character, which lasted longer than an ordinary intermittent. I now began to suspect strongly a local cause in the shape of formation of abscess in some one of the abdominal viscera; but, having no clue (we can get no aid from an African brain) beyond external appearances, manipulation, etc., which threw but little light upon the nature of the case, I could not determine the locality. The *uneasy sensation* in the iliac region had entirely disappeared shortly after the patient called my attention to it. I could discover no enlargement or functional derangement of the liver; the urinary apparatus acted well; the intestinal canal was freely evacuated by purgatives by the mouth, and enemata, until the stools presented a healthy appearance; the stomach retained everything offered it. The tongue was clean, and of natural color, except during fever, in the latter stage of disease, when it assumed the typhoid appearance.

The symptoms continued, the chills becoming gradually less marked, the fever assuming more and more the typhoid character until the 12th of June, when she died.

A post mortem examination of the abdominal viscera four hours after death revealed the following:

Liver natural size, and perfectly healthy. Spleen and kidneys same. Stomach healthy, and partially filled with fluid taken during morning of death; intestinal canal presented here and there slight vascular patches, but no other indication of disease.

On the removal of the intestines the mysterious cause of death was at once revealed by the discovery of an abscess of the right ovary, the uterus and left ovary being normal.

The diseased ovary was about the size of a large hen's egg, and contained about one ounce and a half of purulent matter.

Although the diagnosis in diseases of the ovary, as a general rule, is extremely difficult, had this case occurred in the person of a *white* of ordinary intelligence, there might have been some chance of arriving at a correct conclusion, though, in all probability, the result would have been the same. But as it was, a total inability on the part of the patient to describe her



own feelings, and the symptoms first presented (a fever of well marked remittent character, followed by an equally well marked intermittent of tertian type, unattended with pain or other symptoms calculated to attract the attention to the affected organ) being so foreign to the seat and character of the disease, rendered the diagnosis particularly difficult.

CASE II. In the month of May last I was called to see a negro boy, Harrison, aged 17 years, suffering, as I was informed, from an obstinate *little sore* on the leg. The owner of the boy, a very intelligent planter, seemed to attach little importance to the case, regarding it as a simple ulcer of the skin, but expressed some surprise at the many ineffectual attempts to effect a cure.

I found the leg enveloped in a large cornmeal poultice, on the removal of which, together with a large accumulation of discharged pus, an emaciated limb, with enlarged bone and two openings of circular form with red and thickened edges, and discharging pus, fully enabled me to explain at once to the owner the mysterious cause of the *little sores* not healing.

On examination with the probe my diagnosis at first sight was verified by the discovery of a diseased tibia, (upper third of shaft,) which I at first supposed to be a case of superficial caries, but on closer examination the probe found its way into the interior of the bone by means of two small openings corresponding with the ulcers on the skin. The openings at the time being very small, I could make but a very imperfect examination of the interior, though sufficient to convince me that an operation would be necessary.

I found the patient, a scrofulous subject, much emaciated with disordered stomach, loss of appetite, hectic fevers, disturbed sleep, and other symptoms attending general debility. On inquiry, I found he had been subjected for some time to a treatment of cod liver oil and porter, with poultice to leg. This treatment not meeting with my approbation, I ordered it discontinued. The oil did not agree with the stomach in its irritable condition, and the porter produced, as is often the case, gripings and looseness of the bowels. For the poultice I substituted dressings of lint to absorb the discharge, and the foot and lower portion of the leg being disposed to œdema I applied a bandage.

Under this change the condition of the stomach and bowels improved, and after a week's rest from the "deleterious effects of medicine" I ordered him sulphate of quinine and carbonate of iron — two grains of the former to twelve of the latter, three times daily, a glass of sherry wine be-

fore dinner, and a generous diet. Under this treatment, which I continued for about two weeks, he improved rapidly in strength and general appearance. I now put him on iodide of potassium and sarsaparilla, continuing the wine and generous diet.

He continued to improve until about the middle of June, when he began to complain of *uneasiness*, not amounting to *pain*, in the right lumbar region. This uneasiness not being attended with fever, inflammatory or other symptoms to induce me to suspect anything of a serious nature *internally*, I was induced to attribute it to the muscles of the back being fatigued from the effects of a habit of sitting much in one position. I ordered change of position and moderate exercise in the open air. The uneasy sensation, however, continued to trouble him until he complained of slight pain on pressure, which increased in severity, attended with irregular accessions of light fever, defective appetite, loss of sleep and general irritability of system, until about the tenth day, when for the first time, although I had made daily examinations, I discovered a slight swelling in the back part of the right lumbar region which was quite sore to the touch. The rapid increase of swelling, attended with light fevers, general irritability of system, and an occasional throbbing sensation in the parts, soon rendered the formation of renal abscess evident, and I now directed my efforts to effect an external opening.

On the 11th of July, however, when I had little or no hopes of success in the latter object, but rather anticipated speedy death from peritonitis, the abscess suddenly and most fortunately opened into the ureter, and matter of a light straw color, to the amount of about an ounce, was discharged from the urethra with great relief to the long continued sufferings of the patient.

My attention, as a matter of course, was now principally directed to the kidney, though, in the then emaciated and debilitated condition of the patient, with little hopes of a favorable issue.

I put him again on iron and quinine, fifteen grains of the former to two of the latter, three times a day, continuing wine and generous diet. Fortunately his appetite gradually returned and his general health and strength improved. The abscess continued to discharge a gradually diminished quantity for about three weeks, after which no further trace of matter was discovered and the parts to all appearance, assumed their normal condition.

I may here remark that the discharge from the leg, which, previous to the renal attack, was profuse, gradually diminished in quantity as the ab-

cess advanced, and was as gradually restored as the discharge from the abscess diminished.

The object now was to get the system in condition to warrant an operation on the leg, which, by change of remedies from iron and quinine to potassium and sarsaparilla, and *vice versa*, generous diet and moderate exercise in the open air, was by degrees effected, and, accordingly, on the 28th of August, by a curved incision, four and a half inches long, the bone was exposed, and with the chisel an opening made sufficiently large to extract from the interior several longitudinal pieces of detached bone measuring from a quarter to an inch and a half in length, attended with a discharge of thick yellow pus.

Having extracted all the loose bone I could discover, I partially closed the wound by means of sutures and adhesive strips, leaving open that portion immediately over the opening in the bone, in order to allow free exit to the discharge, and in case of further detached pieces of bone, to admit of removal.

It is now fifteen days since the operation. The general health of the patient has improved and continues to improve wonderfully. The wound is healing kindly, and the discharge gradually diminishing and assuming a more healthy appearance. In fine, everything promises a speedy recovery.

ROBERT HAGAN.

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## Resection of Superior Maxillary Bones:

*Operation performed by M. SCHUPPERT, M. D., New Orleans.*

Frederick Welschhaus — aged 37 years — tailor, native of Germany — for the last eight years a resident of New Orleans — bilious temperament — born of healthy parents — of robust constitution, and enjoyed fine health until the past two years — served four years in the French army in Africa — has never been afflicted with venereal disease of any kind — had hæmorrhoids for several years, which disappeared with the commencement of the disease in question. After nearly two years continual suffering, he placed himself under my care, on the 13th August, 1855, with an extensively developed medullary fungus of both superior maxillary bones. The history of this disease is as follows :

In November, 1853, he had a cough, accompanied with catarrh of the Schneiderian membrane. No treatment was resorted to, and the symptoms abated. Soon, however, he grew worse, and his nostrils became obstructed by a very tenacious mucus, which could very seldom be removed, and then only for a few moments. He suffered greatly from headache, and towards the beginning of spring, pus mixed with blood, flowed from the nostrils. The headache steadily increased, and summer came before he applied for medical aid. He applied to an apothecary, who advised cold lotions to the head. He grew worse, however, and on the 9th September was obliged to take to his bed. Dr. — was called in, and, after a "careful examination," ordered injections of a *decoction* of *flaxseed*. Meanwhile the swelling of the place increased, and cataplasms of the everlasting flaxseed, together with "eau blanc," were ordered. Notwithstanding this heroic treatment, the disease and the patient's sufferings increased. He proposed a consultation, but Dr. — declined, assuring him of his own infallibility in such cases. Time rolled on, and things grew worse, the patient experiencing no relief during day nor night. At last, his patience showing signs of exhaustion, Dr. — made an incision in the right side of the face, between the lip and jawbone, from the middle molar tooth to the middle incisor, for the avowed purpose of opening an abscess. No pus followed this bold surgical invasion, and the wound healed. An incision was now made into the almost bursting skin of the patient's face, and—*mirabile dictu!*—an immense quantity of pus followed. The superior maxillary bone was at this time so far diseased that six teeth were very much loosened; but the doctor assured the patient that they would become fixed again. However, not long after this he visited the patient, accompanied by a dentist. After due consultation, the dentist extracted the six loosened teeth and removed a considerable portion of the alveolar processes, and finished by applying tincture of iodine to the parts. During the subsequent fourteen days there was a continual oozing of matter so offensive that no one could stay in the room with the patient. The dentist now insisted on "an operation," assuring the patient that he was affected with caries of the bone. Dr. — disputed for some time, but finally yielded his opinion. The day arrived, and the patient was seated in the operating chair at the office of the dentist. Before operating, however, a last examination and consultation was had, the result of which was the restoration of the knife to its case, and a recommendation to the patient to seek other and better counsel!



The poor fellow now applied to a member of the profession, who, he says, "is a man occupying an elevated position in society. This gentleman told him he could effect a cure without an operation, and put him under treatment. Until the end of June, 1855, the patient visited this physician, but grew gradually worse, and at last became so weak that he could not pay his visits without assistance. The swelling of the right cheek increased four-fold; on the inner part of the mouth, from the upper jaw bone, a tumor began to show itself; terrible pains in the head and face prostrated him beyond measure, until he at last urged his physician to operate, which was, however, declined. Becoming more helpless than ever, he sent his wife to ask once more of his physician if there was any chance left to be cured? She was assured there was *none*. After several months more of suffering, (the disease increasing and the suffering augmented,) another surgeon of eminence was consulted, but refused to operate. A renowned advertising quack was next consulted, but failed to effect any relief.

At this time, (August 13th,) I was on a visit to a house adjoining the tailor's and was asked to take a look at him. I had never seen him before, nor do I know, personally, anything of the proceedings just narrated. On being asked for my opinion, I stated to the patient that his disease was of a malignant nature, and, to overcome his then pitiable condition, an operation was the only resort. Even should it do him only temporary good, it was the only hope of his being relieved from his terrible sufferings and the state so intolerable to himself and his family. Being asked if I would perform the operation, I declared my willingness to undertake it.

I ordered the patient to the Charity Hospital, on the 15th August, and operated next day in the amphitheatre of that institution, in presence of a number of medical gentlemen, with the assistance of Drs. Beard and Chopin.

The appearance of the disease at that time was as follows: On the right side of the face, below the orbit, between the zygomatic process and the nose, there was a tumor of 5<sup>cm</sup>. in diameter at its base. The skin over it was red, and innumerable enlarged blood vessels were to be seen just under the surface. Above the tumor there was an abscess of the lachrymal sac, and there was no doubt that the disease had extended to the left maxillary bone. Out of the nostrils protruded some tumors. Inside the mouth nearly the whole bony wall was involved in fungus growths of different sizes, of red color, and effusing matter of a penetrating odor.

The patient exhibited undaunted courage as he seated himself in the operating chair. He was put under the influence of chloroform. An incision was now made down the nose, beginning opposite the inner canthus of the eye, having the septum to the left, and cutting through the upper lip near the right edge of the fossa. A second incision was made, beginning at the same point as the first, and extending under the eye-lid to the molar bone of the right side of the face; and a third was made on the left side, of the same extent. The two flaps were now detached from the bones and held back by assistants. The greater part of the right superior maxillary bone was gone and replaced by fungus growth. From the left maxillary bone about two-thirds were taken with Liston's process. The flaps were brought back to their places and retained by twisted sutures. Patient lost but little blood. At night he had slight fever. Morphine was ordered.

On the 5th day the needles were taken out, and notwithstanding the parts had healed, adhesive strips were applied to support the cicatrices. On the 10th day the patient was sent out of the hospital. His appetite had returned; he could sleep well, and his strength was being rapidly regained.

During the operation the patient was under the influence of chloroform up to the time of the application of the cautery. Notwithstanding the blood was constantly running into the fauces during the operation, it was promptly rejected by the involuntary muscular motion.

The many failures which have accompanied this operation, in diseases of the kind under consideration, are well known, and few cases are on record where a permanent cure has been effected. Yet, if there was not a single case recorded, we would consider ourself justified in having undertaken the operation. Where is the physician who, in the most desperate cases, rejects the means of mitigating, at least, the miserable condition of his patient? And why should surgery adopt other principles in great emergencies? For our own part, we reject all other views, and adhere to the maxim "*aux grands maux les grand remèdes.*"

Four weeks have elapsed since the operation was performed, and the patient is not only able to attend to his daily labors, but he has been during that time restored to society, and these results alone are a full equivalent for the few hours of suffering consequent on the operation.

In closing this brief account of a terrible case we confess that the picture before our eyes has stamped on our mind an impression as instructive as painful.

## Norfolk Correspondence.

NORFOLK, VA., 14th September, 1855.

*Messrs Editors:* — According to promise, I shall endeavor to give you a brief sketch of the terrible epidemic that is now devastating this beautiful city. Your worthy colleague, Dr. Beard, and I, arrived here on Saturday evening the 25th August, and were most cordially welcomed, both by the medical profession and the citizens generally. We were grieved to learn, however, that the Mayor, Mr. Woodis, was extremely ill with the fever, and on the following morning he died. The untimely death of the Mayor, caused us some little embarrassment, as our credentials were chiefly addressed to him, but we were assured on all sides that our visit was most opportune, and that our professional services would be most gratefully accepted.

We found the remaining inhabitants of both Portsmouth and Norfolk in a state of complete consternation, at the dreadful ravages of the pestilence, and all business was stopped, save that of administering to the sick and burying the dead. We were told that much the greater part of the population had fled from Norfolk after the epidemic was declared, and those who were compelled to remain, lived in continued apprehension of attack. We took up our abode at the *National*, the only hotel open in Norfolk, and the general rendezvous of both citizens and visitors. We here met with our townsman, Dr. Thos. Penniston, who had come to Virginia, his native State, to spend a pleasant summer, but on hearing the cry of distress that went forth from Norfolk and Portsmouth, he nobly came to the rescue, and was doing every thing in his power to relieve the sick. Dr. Stone, too, of our city had called here for a few days, on his way to the North and did good service by going round with the resident physicians, and instructing them how to treat and nurse their patients. It must be recollected that the physicians of Norfolk, although a highly intelligent and well educated faculty, had little or no experience in the treatment of yellow fever, not one of them having ever been through an epidemic. Under such circumstances, it may be readily imagined what embarrassment they would feel, not only in the *treatment*, but also the *nursing* of patients in this disease. We also found actively engaged here, Dr. Freeman, of Philadelphia, who said he had seen a great deal of the disease in the West Indies, and had it there himself, but had never devoted much attention to its treat-

ment. The Doctor was indefatigable in his exertions, and certainly did *good service*. Such was the extent of *experience* that had been offered in aid of the Norfolk physicians previous to our arrival.

A temporary hospital had been established some three or four miles from the city, the act of removing patients to which probably caused the loss of more lives than were saved after getting there; and, in addition, the United States Naval Hospital at Portsmouth, a splendid establishment, was thrown open for the reception of the sick; but, on account of its remoteness from both towns, it was liable to the same objection as the first named; the patients were greatly injured, if not ruined by the effort to get there. The epidemic was rapidly spreading, and the number of sick, of all classes, was already so great, that it was impossible for them to be properly attended to at their homes.

As for experienced nurses, there were but two or three at that time in the city, and they had but recently arrived.

On the 27th of August, Dr. Beard and I were invited to attend a joint meeting of the Howard Association, Board of Health and City Government, called for the purpose of considering the state of the public health, and devising some more efficient means of affording relief to the afflicted citizens, especially the *poor*. Being called on for our views, we suggested the immediate establishment of a large Hospital, in a central part of the city, for the reception and treatment of all who might apply for admission; and farther, that the poor who could not be well attended at home, should be taken to the hospital *volens volens*. Our suggestion was adopted, and a committee was appointed with full power to carry it into effect as speedily as possible. We were requested to co-operate with this committee in the selection of a suitable building, to superintend its organization, and take entire control of its medical management. The old City Hotel, on Main street, which had recently been evacuated, was obtained, and found to be admirably adapted to the purpose. By the extraordinary efforts of the committee, it was very soon cleaned and fitted up, and in less than three days, we were prepared to receive patients. Fifteen were admitted the first day, and for a short time the influx was as rapid as accommodations could be prepared. A few days after we got under way, a number of physicians, experienced in yellow fever, arrived from other Southern cities, to whom wards were distributed with unlimited control. Those who first entered on duty, were Dr. Huger, of Charleston, Dr. Reid, of Savannah, and Dr. Campbell, of New Orleans. The latter, however, soon asked



to be relieved, and Dr. Miller, of Mobile, who arrived about that time, kindly took his place. A few days since, Dr. Hughes returned home, and Dr. Skrine, of Charleston, took his wards. Dr. Beard left Norfolk about the 5th September, and I only retained to myself some small rooms, appropriated to the better class of patients, and also, the large *negro ward*. We very soon had the hospital equipped with every desirable convenience, and it then went on very smoothly.

The largest number of patients in the house at any one time, was about seventy-five.

The whole number of admissions, from the 29th August, when it commenced, up to the 14th September, the day before I left, was about one hundred and ninety-three, of which, one hundred and forty-three were white, and fifty negroes.

Total number of deaths, sixty nine, of which, *three* were colored. One of these entered in a hopeless state; one was an intemperate man, and one a bright mulatto girl.

The total number discharged was seventy-eight.

The number remaining in the house was forty-six; of which, thirty-one were whites, and fifteen blacks.

The mortality was thirty-four and three quarter per cent., or one 2.80 of the admissions.

This rate of mortality will compare most favorably with any yellow fever hospital that ever was opened in any part of the world; and when we consider that here, as at the Charity of New Orleans, patients were admitted in all stages of the disease, some of them actually *moribund*, and that nearly half of the deaths occurred *within thirty hours after admission*, the general result will appear still more satisfactory. It is but just to say, that the credit of these very favorable results, is chiefly due to those physicians who had charge of the principal wards, and I take pleasure in testifying to the kind and vigilant attention they bestowed upon them. There came on from Savannah and Charleston, a number of second course medical students, who rendered important assistance in the wards, and in conducting the apothecary department. I am sorry to have to add, that several of these noble young fellows are now down with the fever, and two of them in a dangerous state. In the management of my wards, I received much assistance from young Dr. Bignon, of Augusta, Ga., who was with us at the Charity Hospital last winter.

So much for the new *Howard Infirmary*, which, notwithstanding the

favorable results just presented, was signalized as a "*a slaughter-house*" by a correspondent of one of the Petersburg papers. Indeed, it was somewhat remarkable to see in the newspapers, the strange and exaggerated reports that were put in circulation by their Norfolk correspondents. *Dame rumor* seems to have reveled in the calamities that overwhelmed this ill-fated city; multiplying the horrors of the epidemic, killing off people, bringing them to life, and then slaying them over again, thus harrowing up the feelings of distant relations and friends. The naked truth and sad realities were bad enough, without being heightened by fancy or falsehood.

When I arrived in Norfolk, the epidemic was increasing with frightful rapidity, every day adding many new cases to the already enormous list. Some of the physicians told me they had from fifty to seventy-five patients on their visiting list. It was almost impossible to obtain carriages enough, and the fatigue was consequently very great. Fortunately, benevolent physicians and nurses continued to come in from all directions, in sufficient number to supply the increasing demand for medical aid; but, on the other hand, many of these noble spirits were themselves liable to take the disease, and in a short time they began to succumb. Among all the distressing scenes around me, I witnessed no objects more worthy of sympathy than those benevolent physicians and nurses, who had rushed into the midst of imminent danger, with the hope of saving some of their fellow-beings. Cases of this kind became so numerous, and the danger so evident, that the editors of newspapers, before they all suspended, issued separate warnings to all unacclimated persons, and advised them to stay away, as they could do but little good; but it was of no use; they continued to come in, regardless of danger, and were almost invariably attacked shortly after arriving. Thus perished Dr. P. C. Gooch, of Richmond, a gentleman of fine attainments, and founder of the *Virginia Stethoscope*, Dr. Smith, of Columbia, Pa., a young physician, who had gained considerable distinction in a severe epidemic of cholera that prevailed there last year, and Dr. Craycroft, of Philadelphia, a very clever young man. Many others were attacked, but had the good fortune to recover. Among these, were Dr. Morris, of Baltimore, a gentleman, who came early in the epidemic, having resolved to risk his life in learning to combat a disease, which might soon visit his own beloved city. He did a very large practice before he was taken down, and fortunately, had but a mild attack. Dr. Marsh, of Philadelphia, had yellow fever fifteen years ago, at Apalachicola, and thought himself safe; but he was attacked. And so likewise, Dr. West, of New York, was attacked.

notwithstanding he had been in Savannah during the terrible epidemic of last year. A number of visitors were attacked, who had suffered the disease at other places. Among these, were some of the medical students from Savannah and Charleston. We had a case in the hospital of an old man, who said he had the disease in 1796. He was one of the very few old men who recovered from this epidemic. As in New Orleans in 1853, several persons had two attacks of this fever, though, the first was generally mild.

The resident physicians of both Norfolk and Portsmouth, suffered severely from the epidemic, no less than seven having died in the former place, viz: Drs. Higgins, Constable, Hulson, Briggs, Nash, and the two Drs. Sylvester; and three in Portsmouth, Dr. Trugin, and two others. Dr. William Selden, and Dr. Schoolfield, have been attacked, and recovered. But five of the Norfolk physicians have escaped thus far, and two of these have suspended practice on account of affliction in their own families. Drs. Moore, Wright, and Henry Selden, are the only ones who have kept going all the time. The rest of the practice is attended to by strangers.

*General Character of the Disease:* — Although there was a great number of mild attacks that yielded readily to treatment, I think the epidemic may be said to be one of the severest and most fatal ever witnessed. Black vomit was commonly observed in fatal cases, though there were numerous recoveries, especially in young persons, after the appearance of this usually fatal symptom. Uterine hæmorrhage was exceedingly common, but other hæmorrhages were more rare than we usually see in New Orleans.

*Suppression of urine* was exceedingly common in the latter stages, and almost invariably a fatal symptom.

The febrile excitement was generally of a *low grade* for yellow fever, and sanguineous depletion was but seldom strongly indicated; yet, I have no doubt that many cases would have been benefited by the more free use of cups and leeches than was practiced.

The pains of the head, back and limbs, were less severe, I think, than we commonly observe in New Orleans.

There was a general tendency in the old, or those who had passed the meridian of life, to sink after reaching the critical stage, although the symptoms had been mild from the beginning. There appeared to be a want of recuperative energy in the system, which could not always be acted on by stimulants and nourishment in the hour of need. *Delirium* was often observed, and generally, a bad symptom. Yellowness of the

eyes and skin commonly appeared at the critical stage, and was most intense in severe and fatal cases.

*Treatment.*— Among such a number of physicians as was assembled at Norfolk and Portsmouth from various places, you will not wonder that quite a variety of treatment was pursued. The first and great difficulty labored under by the resident physicians, a highly intelligent and well educated body, arose from a want of familiarity with the *natural aspect and course* of the disease; and the next, a lack of experience in the effects of remedies upon it. We may readily imagine the embarrassments that would necessarily arise from these two sources. It is vain to expect to obtain from books all the information that is wanted in the management of this or any other disease. Much that is readily useful, cannot be expressed either by the *tongue* or *pen*. To know how an ordinary case would progress to a favorable or fatal termination *without any interference of art*—when a case is doing *well* and when *badly*—and above all, when we should stop giving medicine and trust to the *efforts of nature*, can alone be obtained from *observation and experience*. To know how a yellow fever patient should be *nursed*, is a matter of no little importance. Now, the resident physicians of Norfolk and Portsmouth, had never before seen an *epidemic* of yellow fever, and of course, were defective in their knowledge of many of the above particulars. Hence, the visits of Drs. Penniston and Stone, of New Orleans, were so peculiarly opportune at the time they came. They conversed with nearly all the resident physicians, went round with them to see their patients, and gave them valuable clinical instruction on matters both *great* and *small*. The same course was pursued by other experienced physicians as they came in; but there soon appeared an embarrassment of no small magnitude, in the fact, that but few of them, although coming from different places where yellow fever is a common disease, were found to agree either on the method of treatment or nursing. There was a difficulty not to be settled; therefore, each was allowed to pursue his own course, and the resident physicians were at liberty to select from the whole, such theory and practice, as they thought to be most judicious, and appeared most successful.

I may mention one marked discrepancy between the physicians of New Orleans and Charleston. The former, recommended the treatment to be commenced with a hot mustard foot-bath, and a dose of castor oil, or some other mild purgative, merely to evacuate the intestinal canal, and the patient to be covered with a blanket, so as to keep up a continued, though not ex-



cessive perspiration, from the beginning of the attack to the end of the critical period, cold applications to the head, and local depletion, if indicated by the severity of the pain. Whereas, the latter pursued a *cooling plan* of treatment from the beginning: The bowels to be gently evacuated, but febrile excitement was to be kept down by the free application of cold water over the head and body, and the use of *very light covering*; the object being not to keep up a *sweat*, but only a *gentle perspiration*, or merely a *soft skin*. For severe headache, they recommended the free and frequent use of the *cold douche*. They also advised the use of cold drinks throughout. Such is the general plan pursued by the physicians of Charleston, as far as I learned from my friends, Ravenel and Huger, two highly intelligent and accomplished physicians, and I must say, it was approved by Dr. Wilson, of Havana, a physician of extensive experience in this disease. We all, however, concurred more fully in recommending mild remedies in the second and third stages of the disease.

I have only mentioned one discrepancy as worthy of special notice, because, it relates to a *general plan* of managing yellow fever patients. I stated to my professional brethren, that whilst almost every possible variety of practice was pursued by some one or more persons in New Orleans, yet, if there was a single point in which there was a greater concurrence amongst the regular and experienced physicians than any other, it was the propriety of keeping the patient covered with at least one blanket, and sweating freely, though not immoderately, throughout the attack.

In this epidemic, the physicians of Philadelphia and Baltimore, as far as I learned from conversing with Drs. Freeman and Morris, pursued a mild course of treatment. The same may be said of Dr. Reid, of Savannah, and Dr. Miller, of Mobile. I have not time at present to say more about the treatment of the Norfolk epidemic, as you must have this in time for your next number. Perhaps I may recur to it on a future occasion. Of one thing I am pretty sure, which is, that whatever practice was pursued, no one, so far as I learned, had reason to boast of any extraordinary amount of success. When this epidemic shall have passed away from these unfortunate cities, as I trust soon it will, and a full report made of the number of persons *exposed, attacked, escaped, and dead*, I venture to predict the results will be found to substantiate the following remarks of Dr. La Roche, in his great work on yellow fever, just issued from the press. In his chapter on *mortality*, the learned author says: "The reader need scarcely be informed, that the yellow fever, wherever it has assumed the epidemic

form, has fully established its claims to being classed among the most formidable diseases to which the human body is liable. This is true, whether we view it in reference to the changes it very generally occasions in the domestic arrangements of a large portion of the exposed population; to the great sacrifices of interest and comfort it entails on these — the necessary effects of the interruption or cessation of commercial and other pursuits; of the abandonment of home, and of the sundering of ordinary ties and occupations — to the perversion of the better feelings of our nature, to which it too often gives rise; or to the immense loss of life it occasions, as well, proportionately, to the amount of the population at large as to the number of the sick. In this latter respect, no disease, the black plague of the fifteenth century, and the Asiatic cholera in our own days excepted, can compare with it."

A remark upon two more points, will close this already too long and hastily written letter. The *non-communicability* of this disease, would appear to be fully established by the facts, that numerous cases have been taken to Baltimore, Richmond, Petersburg, various country seats, and even to the village of Hamptom, only five miles from Norfolk, whence physicians went to visit them daily, and in not a single instance that I have heard of, has the disease been communicated to the attendants, although they had never had it before.

As to the *origin* of this fatal epidemic, it is somewhat involved in mystery. So far as the facts have as yet been ascertained, it is very doubtful whether it originated entirely from local causes, or from a *matres morbi* imported from the West Indies by the steamship Ben Franklin. But more of this anon!

In closing, allow me to say that nothing could exceed the deep sense of gratitude displayed by the people of these cities, towards their benefactors in their days of trouble. I hope to be with you again by the 15th October.

Yours truly,

E. D. FENNER.

P. S. The number of deaths from yellow fever in Norfolk up to this time, is about 1050; in Portsmouth, about 550.

## Dr. Brickell's Cases.

*Wound of Anterior Tibial Artery.*—May 13th, 1855, Albert, f. m. c., aged 17 years—fell on a broken glass bottle, and received a wound about one and a half inches in length on the lower half of the left leg, and immediately over the track of the anterior tibial artery. It bled very freely at the time, but was not considered at all serious, and was bandaged in the ordinary manner. He pursued his avocation until the evening of the 15th, when, on his return home, it began to bleed very freely through the bandages, and I was sent for. I found him sitting in a chair, very faint from hemorrhage and the floor covered with arterial blood. I removed the bandages from the limb, when there was a free jet from the anterior tibial artery. I had him placed in bed, and at once placed a pyramidal compress of lint over the wound, having first brought the edges as nearly together as possible with adhesive strips. The limb was then bandaged from the toes up to the knee, and cold water was ordered to be applied occasionally over the region of the wound. In two weeks, the patient left his bed with a perfect cicatrix, and there had never been any hemorrhage after my first visit.

There can be no doubt of the importance of recording such cases as the foregoing. It is only by statistical information at last, that an important principle can be established in operative surgery, and we should not be content to satisfy only ourselves whether ligation or compression of an artery be preferable; it is our duty to contribute to the general statistic fund, and thus render the knowledge we may have acquired more valuable.

I have no doubt that a coagulum formed in the wounded artery soon after the reception of the injury, and nature was thus making every effort to repair the injury, but the patient, ignorant of the condition of things, completely baffled her by maintaining the erect posture and exercising freely. I have always felt assured, that many wounds of important arteries will, if properly treated by compression, heal readily and rapidly, and the more serious and sometimes difficult operation of ligation, is thus happily dispensed with. The opponents of the operation of compression are too frequently met with; in this very instance, I was urged by a gentleman of high standing, to apply the ligature, as it was "impossible" for a cure to be otherwise effected.

*Paralysis Relieved.* — A very interesting case in one of the medical wards is that of a nurse, who had for three years been the subject of nearly entire paralysis of the supra-spinatus and deltoid muscles of the right arm, accompanied, as a consequence, by permanent contraction of the teres muscles. The patient says, that about three years ago, he slept soundly for several hours with the affected arm under his head, and fully extended in a line with his body. On awaking, he perceived a considerable degree of numbness in the muscles of the shoulder, with inability to use the limb readily. In a short time, he became unable to raise the elbow more than about ten inches in a direct line from the body, and, although his general health has been good, this state of affairs has existed up to the present time. All the other muscles of the limb are in normal condition, and he uses them with facility, but, of course, this paralysis of the two great muscles for raising the arm, seriously interferes with his duties as a laborer, and has been a source of much trouble to him.

He was ordered nourishing diet, the shoulder to be well rubbed three times daily with an ointment composed of one dram of veratria to the ounce of lard, and the limb to be exercised as much as possible by efforts to raise it, etc. On the third day, he complained of the prickling sensation produced by the veratria, but thought he had rather better use of the limb. The application was persevered in, and at the end of a month, he could with ease, reach objects higher than his head, and expressed himself as having sufficiently good use of the arm for all "practical purposes." The muscles, which, from inaction, had become considerably atrophied, were fast reaching their natural size, and for several weeks past, he has been whitewashing and doing sundry other jobs about the hospital. D. W. B.

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## Editorial and Miscellaneous.

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### TYLER SMITH ON LEUCORRHEA.

In this age of medical book-making we are but too often called on to wade through hundreds of pages without meeting with a single original idea from the brain of the self-styled *author*, and this is one of the most arduous of all editorial duties. However, we are not unfrequently quite repaid for our trouble by the receipt of volumes, the perusal of which is so refreshing to the mind as to dispel



all those harsher feelings aroused in us by having to plod over the compilations every day issued by our professional "hucksters of other men's brains," and amongst this class stands unrivalled the work of Dr. Tyler Smith on "The Pathology and Treatment of Leucorrhœa." Notwithstanding the subject of Leucorrhœa has been so closely studied, in our humble opinion no work on the subject has hitherto been published, which can at all compare with the one in question in point of combined conciseness, originality and intrinsic worth; in fact, it must, for the present at least, stand as authority *par excellence*.

Dr. Smith devotes chapter 1st to a microscopical examination of the parts involved in the production of leucorrhœa, viz: the mucous surfaces of the vagina and os and cervix uteri; he treats very fully of this part of the subject, and convinces the reader of the great importance which belongs thereto. In fact, this distinct demonstration of the anatomical difference between closely adjacent parts is the first great step towards the proper comprehension of the entire subject, leucorrhœa.

Chapter 2d is devoted to an elaborate description of the "glandular structures of the canal of the cervix uteri." After quoting from several authors who have treated most satisfactorily on this subject, Dr. Smith enters on an original description of the parts, commencing with the observation: "A careful examination of the cervix uteri itself will, however, show that even the most careful of these descriptions is imperfect." The author has not been content to follow in the old beaten path of anatomists—satisfied with pointing out the existence of the "penniform rugæ" of the cervix, with "mucous follicles," and "laminae" between them—but he has applied his microscope and found *rugæ* upon *rugæ* and *follicles* upon *follicles*, until he is brought to the declaration: "The crypts in the furrows are still further divided and subdivided, so as to double or treble the number of follicles and laminae seen with the lower power. In a portion of the cervix, comprising only three rugæ, and their two interspaces, upwards of five hundred mucous follicles were easily counted, so that it is within the limits of moderation to say that a well developed virgin cervix uteri must contain at least ten thousand mucous follicles; indeed, even this number is probably greatly exceeded."

The author points out distinctly the great difference between the epithelium of the vagina and os and cervix uteri, and that of the canal of the cervix; while that of the vagina and os and cervix uteri is constantly squamous, that *just* within the os is cylindrical, and that from the remainder of the canal cylindrical and ciliated. So that, by applying the microscope to leucorrhœal discharges we can readily determine the seat of the discharge—a point hitherto unachieved. Of the mucous membrane of the fundus uteri the author says, "however important it may be with respect to the disorders of menstruation, it is seldom, I believe, the source of leucorrhœal discharges."

Chapter 3d is devoted to a consideration of "the secretions of the vagina and of the os and cervix uteri and the cervical canal, in the healthy state."

The author thinks the secretions from the sebaceous follicles of the vulva obviously intended "to defend the vulva from friction, and to preserve the surface

from the irritation of the uterine and vaginal secretions, and of the urine." Of the mucus of the ostium vaginae he says: "The secretions of the mucous glands of the ostium vaginae is said to be connected with the sexual function, and to be increased under excitement. In some women a profuse emission of fluid appears to take place from these glands during sexual intercourse. The secretions from these glands, in the absence of excitement, is so inconsiderable, or it is so mixed up with the scaly epithelium of the mucous surface in this situation, that it is extremely difficult to ascertain precisely its microscopical qualities. Like the other vaginal secretions it has an acid reaction." And finally, "I suspect the importance of these glands with respect to leucorrhea has been greatly overrated."

Of the vaginal mucus the author says: "The mucus of the vaginal canal is not found in any considerable quantity in the healthy subject; it is only secreted in sufficient quantity to keep the mucous surface in a state of lubrication. It lies upon the mucous membrane as a milky fluid, containing quantities of small curdy points or masses, and consists of a transparent or semi-transparent plasma, containing an abundance of scaly epithelium and its *debris*. In the natural condition of the part, the epithelial scales are either mature or beyond the period of maturity and wearing away. But when the vagina is in a state of irritation, the epithelium is shed more rapidly, and cells of all sizes, from mere nuclei up to perfect scales, are found in great quantity in the vaginal secretion."

On account of the various physiological changes which occur during menstruation, pregnancy, parturition and lactation, the author deems it necessary to treat of the secretion of the cervical canal separately in each of these states.

He says that in the healthy and unimpregnated state little or no secretion issues from the cervical canal: "but when the labia uteri are separated, the canal appears to be full of its secretion." This is a clear viscid mucus. On the accession of the catamenia this plug of mucus is washed away, to reappear on the cessation of the discharge. "Thus it would seem to be the function of the glandular structure of the cervix in the unimpregnated uterus, to secrete each month a sufficient quantity of viscid mucus to fill the canal of the cervix, the mucous follicles becoming comparatively inactive when this has been established," etc. "The mucus itself consists of myriads of mucus-corpuscles entangled in a transparent viscid plasma. The plasma is so tenacious, that the mucus-corpuscles are found to be arranged in strings when placed under the microscope, and individual corpuscles are frequently seen to be elongated from the same cause. The use of the cervical mucus is probably two-fold. In the first place it closes the cervix uteri and defends the cavity of the fundus from external agencies as completely as though it were a shut sac. In the second place, it appears to afford a suitable medium for the passage of spermatozoa through the cervix uteri into the uterine cavity."

In pregnancy, the author says that this tenacious mucus persists in the cervix, the plug gradually changing its shape as the uterus expands. "In the upper part of the cervix, it is clear and transparent. The plug consists, in the upper part of the cervix entirely of mucous globules and plasma: but in the lower portions of the plug these elements are mixed with scaly epithelium in considerable quantity."

The author stops here to dwell on a very important point. He says: "In the upper part of the cervix the secretion is alkaline, but the lower part of the plug gives an acid reaction. This acidity is owing to the effect of the acid reactions of the os uteri and vagina, which come in contact with the lowest part of the mucous plug of the cervix. The acid coagulates the alluminous matter of the plug, and it is in this way that the lowest portion is rendered white and almost solid." On this "white plug" the author lays great stress as being one of the surest signs of pregnancy. He says it is rarely present in any other condition, and cites cases in point.

At the commencement of parturition the cervical plug of pregnancy is thrown off in consequence of dilatation of the os, and then follows a free secretion of a thinner mucus from the cervical glands, which lubricates the vagina thoroughly. This mucus is in sufficient quantity and sufficiently alkaline to neutralize the acid secretion of the vagina and give an alkaline character to the vaginal canal during parturition. "Upon the completion of natural labor, these glands continue to secrete with considerable activity, and their secretion forms a part of the lochial discharge. In many cases, the last secretion which appears after the cessation of the lochia, is the viscid secretion of the canal of the cervix. Thus it is, perhaps, during parturition that the glandular function of the canal of the cervix uteri is most actively performed."

The author thinks that the leucorrhœal discharge which frequently takes place during lactation is from the cervix uteri. He thinks that the intimate relation which is known to exist between the uterus and mammæ, as evidenced by the uterine contractions induced by placing the child to the breast in the first few days after its birth, "is preserved to some extent during the whole period of lactation, and I have met with some cases in which cervical leucorrhœal discharges constantly occurred whenever the child sucked the breast." He thinks that the foundation of chronic cervical leucorrhœa is sometimes laid at this time. He says: "In women who are drained largely by leucorrhœal discharges while nursing, it is often only necessary to direct them to wean the child, and the discharge speedily diminishes."

Chapter 4th is devoted to the consideration of "the different forms of leucorrhœa." The author says—"All pathology has its basis in physiology. In the various departments of medicine, the only way of reconciling differences of opinion is by investigation. The demonstration of two very differently organized surfaces in the vagina, and in the canal of the cervix uteri, with the existence of two very distinct forms of secretion, naturally lead us to the consideration of two principal forms of leucorrhœa." After reverting to the difference in the anatomical structure of the vagina and the canal of the cervix, he says—"Leucorrhœa admits of a similar division. The first and the most frequent and important is the mucous variety, consisting chiefly in mucous corpuscles and plasma, and secreted chiefly by the follicular canal of the cervix. The second is the epithelial variety, in which the discharge is vaginal, or is secreted by the vaginal portion of the os and cervix, and consists for the most part of scaly epithelium and its debris. These two varieties may of course exist in various degrees of combination; sometimes the one and sometimes the other preponderates, or is the original affection;

but the chief importance must be given to cervical or mucous leucorrhea, as being the most obstinate and common."

The author then goes on to define explicitly and *in extenso*, the differences between vaginal and cervical leucorrhea, in all their different phases, and is certainly more satisfactory in his descriptions than any other author with whose works we are acquainted. He pursues the subject in all its minuteness — not satisfied with the phenomena as presented to the naked eye, but applying the microscope on all occasions; and he finally reduces the whole thing to the two following formulæ:

*Vaginal or Epithelial Leucorrhea.*

1. Acid Plasma.
2. Scaly Epithelium,
3. Pus-Corpuscles,
4. Blood-Globules,
5. Fatty Matter,

*Cervical or Mucous Leucorrhea.*

1. Alkaline Plasma.
2. Mucous-Corpuscles.
3. Altered Cylinder Epithelium,
4. Pus-Corpuscles,
5. Blood-Globules.
6. Fatty Particles.

Dr. Smith says, it is very difficult to prove that leucorrheal discharges do not sometimes take place from the cavity of the fundus uteri, though, he says, there are many reasons for believing that it rarely occurs. In the first place, "there is no secreting structure in this situation, at all equivalent to the glandular structure of the uterine neck." Again, "Leucorrhea is very common during the whole of pregnancy, and in the latter part of gestation, it is impossible that any of the secretion can be supplied by the mucous membrane of the body of the uterus." Again, "In procidentia uteri, when the os uteri and the lower part of the cervical canal are exposed to the irritation of the external air, the mucous secretion from the cervical glands is almost always profuse, and differs in no respect from the discharge in cervical leucorrhea, except that it is transparent, because there is no admixture of vaginal acid." Again, "On the other hand, in *inversio uteri*, there is no mucous discharge from the exposed mucous membrane of the fundus, but the patient is drained by constant and excessive loss of blood." Again, "In cases of *polypus uteri*, or fibrous tumors in the walls or cavity of the organ, when the fundus uteri is irritated, we observe that sanguineous discharge is almost always present."

Chapter 5th is devoted to the study of the sequelæ of leucorrhea, and chapter 6th to the relations between secondary syphilis and leucorrhea. Both these chapters are replete with valuable suggestions, which our limited space forbid our detailing. Chapter 7th treats of "the relations of vaginal or epithelial leucorrhea to gonorrhea in the female; to urethritis in the male; and to the ophthalmia of new born infants." The author evidently regards the vagina as the peculiar seat of gonorrhea — the os and cervix becoming secondarily affected, and finally, in a few cases, the canal of the cervix becoming involved.

He thinks urethritis in the male is not uncommonly produced by contact with the vagina affected merely with leucorrheal discharge, though, he is at a loss to designate that particular condition of this discharge, which renders it capable of infecting the male urethra.



The author devotes chapter 8th to the anatomy and pathology of the ovula nabothi. He regards these bodies as nothing more than obstructed mucous follicles.

Chapter 9th treats of "the relations of leucorrhea to disorders of the function of menstruation." This is one of the most important points in the study of leucorrhea, and we feel totally inadequate to the task of successfully abbreviating the valuable remarks of the author. We invite the reader's special attention to the chapter.

Not less interesting is chapter 10th, which treats of "the relations of leucorrhea to sterility and abortion." The author says there are two conditions in which leucorrhea causes sterility. "The one consists of those cases in which the function of menstruation and ovulation is disordered, as a secondary result of leucorrhea. The other consists of cases in which the spermatozoa are either prevented from entering the uterus, or are destroyed by the unhealthy utero-vaginal secretions."

The author says, that sterility is frequently the result of menorrhagia as secondary to leucorrhea. Also, "sterility is almost an invariable result when leucorrhea leads to amenorrhea and chlorosis, with complete torpidity of the ovaria and fundus uteri, the function of ovulation and the secretion of the menstrual fluid being both suspended. Instances of sterility are also frequently found in cases of cervical or vaginal leucorrhea, complicated with membranous menstruation. The exceptions are very few in which conception can occur simultaneously with the exudations peculiar to this form of leucorrhea."

But we have run entirely beyond the limits assigned us in this number of our Journal, and abruptly close by most heartily recommending Tyler Smith's work on leucorrhea to all who take the slightest interest in the diseases of females.

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#### QUARANTINE AND YELLOW FEVER AT NATCHEZ.

One of the most interesting and important circumstances connected with the history of yellow fever is its recent appearance at Natchez, Miss. Having suffered from this scourge in 1853 to an extent truly appalling, the community, instigated by one of their most able and accomplished physicians, determined on the establishment of quarantine during the summer of 1854, hoping thus to preserve the health of their city. All accounts would lead us to infer that this quarantine was as rigidly enforced as human efforts would admit of. Natchez escaped an epidemic, and the existence of even a single case during the season was never officially announced or admitted, so far as we are aware. We must not omit to say, however, that one or more of the most intelligent of the Faculty declared the existence of one or more palpable cases, and, if we mistake not, there was considerable excitement and, perhaps, asperity of feeling exhibited by our brethren there through the medium of the newspapers. The absence of anything approaching an epidemic, however, satisfied the community, and they were convinced that they had found in quarantine a guardian angel for all future time.

As soon as yellow fever was announced in New Orleans this summer, the City Council of Natchez determined on the reestablishment of quarantine, and all ac-

counts tend to show that the same was, if possible, more rigidly enforced than it was in 1854. What has been the unhappy result? The following extract from a Natchez paper will show:

“BOARD OF HEALTH.—*City Hall, Sept. 8th, 1855.*—At a called meeting of the Board of Health, held this day, at 3 o'clock, P. M.,—present R. W. Wood, Esq., Mayor; Dr. L. P. Blackburn, Health Officer; Messrs. Barnes and Doniphan,—the following report of Dr. Blackburn, was submitted, which, on motion of Mr. Doniphan, was adopted:

“*To the Board of Health of the City of Natchez:*—Gentlemen: It becomes my painful duty to announce to you the existence of yellow fever in our city.

“Yours respectfully, L. P. BLACKBURN, *Health Officer.*”

“The following preamble and resolution of Mr. Doniphan was, on motion of Dr. Blackburn, unanimously adopted:

“The City Council, with commendable diligence, as soon as yellow fever was announced in New Orleans, established, and have continued to enforce rigidly, the quarantine laws against all boats and their passengers from infected ports, and have, also, had an armed guard on the roads leading to the city, as well as a secret police to detect infractions of the law, and have done everything in their power to keep Natchez free from exotic yellow fever; notwithstanding all which, it is in our midst, and threatening to become epidemic—

“*Therefore resolved,* That the yellow fever now existing among us is a visitation of an All wise Providence, against which the most judicious human efforts have been unavailing; and while we deplore the fact, we feel it our duty to make it known to our citizens, that they may take such measures to avoid the infected atmosphere as they may deem advisable.

“*Resolved,* That the proceedings of this Board be published in hand-bill form and posted on the streets, and also inserted in the papers on Tuesday.

“On motion, the Board then adjourned.

“WM. H. STEWART, *Secretary of Board of Health.*”

We must say that we most heartily sympathize with the community of Natchez, both in their distress and their disappointment, and we sincerely trust that their sufferings this season may be very limited. It is a matter of sincere regret that we have to cite their city as an instance of the failure of rigid quarantine to prevent the ravages of yellow fever. The non-existence of yellow fever in Natchez last year, (taking only official announcement,) was negative evidence in favor of quarantine; the existence of yellow fever this year, is affirmative evidence against it. Is it to be tried again?

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TRANSACTIONS OF THE STATE MEDICAL SOCIETY OF THE STATE OF NEW YORK.—It gives us great pleasure to acknowledge the receipt of a volume of 308 pages, bearing the foregoing title, and accompanied with the compliments of Dr. F. H. Hamilton. It is a matter of regret that our limits forbid our giving an extended notice of the valuable contents of this volume. Our New York brethren are entitled to great praise for their labors, and all the harm we wish them is that they may offer us just such a present every year. He who can command the volume and neglects to read Dr. Hamilton's “Report on Dislocations, with especial reference to their results,” certainly does himself great injustice.

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ECLECTIC.—We will take it as an especial favor if “Eclectic” schools, editors, etc., will not in future encumber our box at the Postoffice with their various prints. We are very much obliged for their attention, but being “fish of a different water,” we have no earthly use for their favors.

## HEALTH OF OUR CITY.

We have little to say under this head. Our annexed table of mortality shows clearly that yellow fever is fast disappearing, and the mortality by all other diseases has been gradually declining. Our absentees are flocking back rather too rapidly however. Having kept clear of the pestilence all the summer, it would be rather more prudent to wait a week or ten days longer.

*List of Mortality for Four Weeks, ending Sept. 23, 1854.*

	1st w'k.	2d w'k.	3d w'k.	4th w'k.	Total.
Total number of Deaths.....	407	373	283	214	1277
Males, .....	272	257	203	124	856
Females, .....	126	108	75	87	396
Sex not stated,.....	9	8	5	3	25
Whites, .....	375	349	264	199	1187
Blacks, .....	14	10	9	7	40
Mulattos, .....	15	11	9	6	41
Color not stated, .....	3	3	1	2	9
Native Americans,.....	33	31	39	37	140
Northern States,.....	9	11	5	6	31
Western States,.....	3	5	4	7	19
Southern States, .....	21	15	30	24	90
Foreigners, .....	252	214	151	109	726
English, .....	11	6	8	7	32
Irish, .....	75	73	47	42	237
French, .....	46	42	37	30	155
Germans, .....	99	83	50	30	262
Place of Birth not stated,.....	122	88	93	68	371
Age not stated,.....	26	21	32	23	102
Under one month old,.....	16	11	17	13	57
From one to three years,.....	26	20	38	27	111
From five to ten years,.....	10	8	8	4	30
From ten to twenty years,.....	55	48	25	15	143
From twenty to thirty years,.....	163	156	81	73	493
From thirty to forty years,.....	70	74	41	31	216
From forty to fifty years,.....	17	15	25	20	77
From fifty to sixty years,.....	13	11	8	3	35
From sixty to seventy years,.....	8	5	4	2	19
From seventy to eighty years,.....	3	4	4	3	14
From eighty to ninety years,.....	0	0	0	0	0
Over ninety years,.....	0	0	0	0	0
Yellow Fever,.....	301	255	150	89	795
Other Diseases, .....	88	91	112	105	396
Typhoid Fever,.....	1	3	4	5	13
Cholera, .....	3	4	5	4	16
Intemperance, .....	2	3	1	2	8
Accidental, .....	1	2	3	2	8
Still-Born, .....	5	7	6	5	23
Diseases not stated,.....	6	8	2	2	18

## CHARITY HOSPITAL REPORT — For the month of October.

Admitted, - - - - -	1,085
Discharged, - - - - -	867
Died, - - - - -	304
Remaining under treatment, - - - - -	439
Births, — Males, 5; Females, 1; Still-born, 4. Total, 10.	

ATLANTA MEDICAL AND SURGICAL JOURNAL.—We are in receipt of the first number of this new journal, edited by Professors J. P. Logan and W. F. Westmoreland. If we are allowed to judge from the number before us, we would at once say that the efforts of the editors must be crowned with success. We wish them the fullest realization of their hopes, and cordially welcome their journal to our exchange list.

TO OUR SUBSCRIBERS.—Those of our subscribers who are in arrears must pardon us for reminding them that we have to pay regularly and promptly for everything connected with the publication of our journal, and on our patrons we rely for the means of meeting these expenses. The editor's life is hard enough without taxing his private purse for the purpose of furnishing others a medical journal. We have no idea of making money by our journal; on the contrary, if our patrons were to pay us to-morrow every dollar now due, we should still be largely "out of pocket."

AN APOLOGY.—One or two numbers of our Journal have been rather late. For this we plead sickness and its concomitant troubles, and venture to promise better things in future.

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## Excerpta.

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LECTURE ON LITHOTOMY AND LITHOTRITY. — *By James Syme, Esq.* — The last case in which I performed lithotomy is also doing well. During the first two or three days after the removal of the tube, there was a little bleeding, which is, however, never serious at that period, and, in the present case, appeared to be owing to a sort of struggle of the urine to pass by the natural passage, instead of through the wound, which being small at its deep part, and little more than a simple incision, on account of the small size of the stone, tended to close thus early. I, therefore, reintroduced the tube, which was retained for three days, and without stuffing the wound, had the effect of removing the tendency to hæmorrhage, as well as the painful desire of micturition which previously existed.

I may take this opportunity of saying a few words regarding a matter that is still unsettled in surgical practice, and about which it is right that an opinion should be expressed—viz: the comparative advantages of the operations of cutting and crushing for stone. The objections to cutting are chiefly the pain and danger connected with it, and the horror attending cutting and bleeding. So long as the operation was conducted on uncertain principles, as when we had Mr. Martineau of Norwich, who lost only two out of eighty-four cases, telling us to cut freely into the neck of the bladder, while another very successful lithotomist—Mr. Liston—inculcated that the less the prostate was cut the greater would be the patient's safety,—when such conflicting statements were made by men regarded as authorities on the subject, the operation thus beset with uncertainty could not but be attended with danger. But now, when we know that there is a certain part which requires to be cut, and if this and no more be divided, the ope-



ration *per se* may be said to be devoid of danger; and now that these things have been so clearly laid down, that any man who possesses ordinary anatomical knowledge and surgical skill can do what is necessary with precision, the argument regarding the danger of lithotomy may be dismissed. There is no operation absolutely free from danger; but I think we may say that this one, if performed on correct principles, in favorable circumstances, is almost as safe as any that could be named. If the stone be large, or the prostrate diseased, lithotomy is dangerous; but you observe, that the question does not regard such cases, since crushing is only employed when the stone is of moderate size and the parts healthy. The argument that lithotomy is more painful than lithotripsy has no force at the present day, when the patient, on waking up from the sleep of chloroform, experiences little uneasiness, to interfere with the relief at once derived from the operation. In fact, the chief claim to preference in favor of lithotripsy is, its freedom from the horror attending cutting and bleeding. Patients have naturally a strong aversion to the knife and to loss of blood, but this must not be allowed to weigh against comparative safety and facility of performance. If the stone is very small, it may sometimes be crushed at once, and the patient quickly relieved of all symptoms; I have myself succeeded in this way, as others have done, but it must be admitted that such an occurrence is rare; generally, the process requires to be repeated, and the oftener this is done, the greater is the risk of a result by no means uncommon after lithotomy—viz: a chronic inflammation of the bladder, with symptoms far more serious and intractable than those of stone itself. It also appears, that if the instruments are frequently introduced and withdrawn, the sensitive ring at the base of the prostrate is apt to suffer some injury, attended with distressing symptoms. When that part of the prostrate has yielded so that an instrument, instead of going into the bladder with a jump or jolt, passes in perfectly smoothly, it might be supposed that the patient would pass urine with even greater facility than natural, but, on the contrary, it is found that micturition is then apt to become attended with much difficulty. The first case of lithotripsy here, occurred under the care of Mr. Liston, and the man left the hospital, as it was thought, in a satisfactory condition, but the same patient came afterwards under my care, (Mr. Liston being then in London,) in a state of extreme distress, which he said had existed ever since the operation, being neither able to pass his urine nor keep it. I never saw a patient suffer more from his urine than that man, who finally died, worn out by his miserable condition, yet, on dissection of the body, I found the bladder sound, and no appearance of anything wrong, except that the junction of the prostrate and neck of the bladder was unnaturally expanded. If a fragment of a calculus is left behind after lithotripsy, it forms a nucleus for the deposition of another stone, and if irritability of the bladder has been excited by the operation, the urine remains ammoniacal, and readily deposits earthy matter, so that the calculus grows with great rapidity. I may mention as a curious fact, that the second case in which Mr. Liston employed lithotripsy in this hospital, was also afterwards admitted here, under my care, when I cut him, and extracted a stone so large, as to show that it had been growing extremely fast. I may add, that lithotripsy is not even free from the danger of a directly fatal result; no one contends that it is so, but only that it is less dangerous than lithotomy. But in estimating the statistics by which it is attempted to establish this position, you must remember that lithotripsy is employed only in selecting cases, while lithotomy is used for every variety: and that lithotripsy, while not exempt from immediate danger, exposes to great risk of leaving fragments behind, and of exciting irritability of the bladder, or other distressing urinary symptoms. A patient on leaving the hospital, apparently well, is set down as cured, and may remain well, so far as the hospital statistics are concerned, whatever may be his subsequent condition; and in private practice, when a patient pays his fee, and takes himself off to the place whence he came, the surgeon may believe that he is permanently cured; but in order to form a true estimate, you must take into account the after history. Several patients have applied to me with stones of moderate size, and not liking my advice to have them cut out, have gone to London, and there undergone lithotripsy. Of these, the first to which I will allude was a gentleman in the middle period of life, with a small stone, who went to Mr. Liston, and turned out an emi-

nently successful case of lithotrity, having continued well ever since. This gentleman was so much pleased with his recovery, that he sent up to Mr. Liston one of his farm servants, a fine, healthy man, who was admitted into University College Hospital, and died in a few days after the crushing of the stone. Four other patients have since gone to London under the circumstance I have mentioned. Of these, the first was cured, and the relief has been, so far as is known to me, complete and permanent; the second died a few days after the operation; the third was the subject of a long and anxious attendance, for he himself told me that the operator paid him a hundred visits, and that patient never knew any comfort of life afterwards, being constantly tormented with a desire to pass urine. yet, I know that this was considered a very successful case; the fourth case, that of a gentleman with a small calculus, was another brilliant success, yet, if I may trust his own assurance, he never knew comfort afterwards, but traveled about, at home and abroad, using one mineral water after another, and consulting various practitioners, one of whom attempted to wash away, with muriatic acid, a calcareous incrustation, which he supposed to exist upon his bladder. When, therefore, you read the statistics of lithotrity, you should bear in mind that the cases may have a subsequent history, respecting which no information is afforded you.

My opinion regarding lithotrity is, that while some patients will get off more easily by it, than by cutting, yet, on the whole, it is less satisfactory. Take twelve patients with stones suitable for crushing, and treat them all in this way; some will, perhaps, get off without any further trouble; but of the remainder, some will suffer from irritability of the bladder; some will have a return of the symptoms, in consequence of fragments having been left to form the nuclei for subsequent concretions; while others will be set free from further trouble by death occurring within a few days after the operation. You must also take into account that there is much more practice required, in order to perform lithotrity properly, than is necessary for lithotomy, in which, ordinary surgical skill is sufficient, and that it is only in the hands of the most expert operators, that lithotrity has even the degree of safety which I have admitted, and that otherwise, it is far more dangerous than lithotomy, so, that every man who aspires at learning this art, must lay his account with a great deal of discomfort in his early cases. On the whole, therefore, I am of opinion that the wholesome, effectual, and, I will add also, safe method of excision should in general be preferred to crushing. — *London Lancet*.

CYANURET OF MERCURY. — M. Desmarts, of Bordeaux, after a careful comparison of the effects produced by the different preparations of mercury, has come to the conclusion that the cyanuret is superior to all others, especially in syphilis. He believes it at the same time efficacious and innocuous in its action: he never saw it occasion salivation or any intestinal irritation: and often, when all the preparations of the metal had failed to produce benefit, he has seen it restore to health patients whose cases seemed hopeless in the extreme. He has found its use to be efficacious in certain cases where the patients had suffered, for a long period, obscure pains, for which no cause could be discovered. He has employed it with benefit in iritis, and in syphilitic affections of the nose and fauces. — *Dublin Hospital Gazette*, April 1, 1855.

NITRATE OF SILVER FOR THE CURE OF PROLAPSUS ANI. — Mr. Lloyd treats prolapsus ani by smearing the whole surface of the protruded bowel with solid caustic, and then returning the bowel. The application is repeated once in a week or fortnight, as may be requisite. Mr. Lloyd states that he rarely found it necessary to employ it more than three or four times; and further, that although the plan had been one invariable resort with him for a long series of years, that he had never known any untoward consequences to result. In cases in which the protruded bowel has become swollen, and is difficult of reduction, the effect of the caustic is surprising. In one such case, the mass could be easily seen to diminish in size under its influence. Mr. Lloyd does not limit the use of this remedy solely to prolapsus, but adopts it also in cases of hæmorrhoidal congestion, and thickening of the mucous membrane about the verge of the anus. — *Med. Times and Gaz.*

**SUICIDE WITH TINCTURE OF ACONITE.**—On the 22nd of May, 1852, an inquest was held on a young woman name J. T——, a domestic servant, who had died in University College Hospital on the 19th of the same month. It appeared from the evidence given, that the deceased had, on the last-named day, risen up hastily from her tea, which she was taking with a fellow-servant, between six and seven o'clock in the evening; that she had gone up stairs, and soon afterwards returned with a small phial, capable of holding somewhat more than two drachms of fluid, and which she threw on the kitchen table, stating that she had drunk its contents. A surgeon was soon afterwards sent for, who administered an emetic of salt-and-water, and then frequent draughts of warm water, in order to evacuate the stomach. She was said to have been continually retching, or making unsuccessful efforts to vomit, from about twenty minutes after taking the poison, and by the advice of the surgeon she was removed to the hospital, about eleven o'clock on the same night.

Mr. Henry Simpson, physician's assistant at University College Hospital, deposed that towards half-past eleven on the night in question, the deceased had been received there. The following account is transcribed from the notes of the case made by Mr. Simpson:

“At the time of her admission, she was in a state of great prostration; the pulse was imperceptible at the wrist, the action of the heart scarcely audible, the skin cold and clammy, the eyes staring. She was, however, conscious, and appeared distressed, and as if she did not wish to be interfered with. She was placed in bed, hot bottles were applied to the feet, and mustard cataplasms to the calves, etc. We tried to give her ammonia and brandy, but she took very little, the jaws remaining so closely fixed together, (apparently from a voluntary determination of the patient to admit of no assistance,) that the attendants were obliged to force them apart, in order that the stimulants might be administered. At this time, about a quarter to twelve, the pupils were dilated, the right one being nearly oval, and the left irregularly polygonal. She remained quite conscious until her death, at ten minutes to twelve, which took place in the following manner: Having resisted for a while the attempts to give her medicine, she suddenly sat up in bed, and said, “I will take more if you wont force me.” Having then tried to swallow a little, she sank back again: the heart's action was no longer perceptible by the aid of the stethoscope, and after this she gave but one inspiratory heave, her death thus occurring somewhat more than five hours from the time when the poison had been taken. In the progress of the treatment, no observations were made as to any loss of *sensibility* in the limbs, but the preceding account shows that the intellect and power of voluntary motion were not materially impaired. No vomiting occurred after her admission into the hospital. There was not, up to her death, the least tendency to sleep, or any symptoms proving a narcotic effect of the drug. The lower part of the trunk was observed to be stained with dark-colored feces.”

The post-mortem examination was made by Mr. Simpson, about fourteen hours after death. Externally, the body was seen to be pale and bloodless, but with some slight discoloration about the neck and one of the legs. Both pupils were slightly dilated. On opening the head, neither the dura mater nor the vessels of the brain appeared to be congested; the superior longitudinal sinus contained very little blood; the brain looked healthy; a small quantity of serum was found in its ventricles: no blood was extravasated in or upon any portion of that organ. In the chest, the heart was seen to be in a healthy condition, with the exception of a slight thickening of the valves on its left side; there was dark fluid blood in both ventricles. The lungs were not unusually congested, and were otherwise healthy, as were the great vessels. No extraordinary quantity of fluid was found effused in the cavities of the serous membranes. Abdomen: the liver and spleen were healthy; there were traces of old inflammation in the left kidney, which was somewhat contracted. No sign of pregnancy existed: the uterus and peritoneal ligaments were healthy: both ovaries, however, contained cysts. The mucous membrane of the pharynx was found slightly redder than natural: the œsophagus pale. The stomach was large and flaccid; both of its ends were tied while *in situ*, and on its removal from the body, its contents were carefully emptied into a vessel



for examination, when its lining membrane was found paler than natural, except along the greater curvature, where some patches of congestion were found, not, however, distinctly of an inflammatory character. The intestines were healthy throughout. The fluid on the lining membrane of the stomach, when touched with the tip of the finger and applied to the tongue, imparted distinctly the peculiar sensation given by aconite. The contents of the stomach were evaporated down on a water bath, and treated with pure alcohol; and on the solution being filtered, the taste of aconite was again unequivocally obtained.

*Remarks.* — Very few instances of poisoning by aconite are to be met with in books, and we believe that the one just related, is the first recorded case of suicide by means of that agent in this country. As up to this time, no chemical test has been indicated as distinctive of aconitina, and the persistent tingling and burning sensation on the tongue, is the only known means of detection of this substance, a great difficulty in stating that poisoning has happened in consequence of its employment is liable to occur. In the present case, however, that difficulty was satisfactorily overcome at the inquest: the persons of whom the material had been purchased, had frankly admitted that it was in fact tincture of aconite. It was elicited that about two years previously, a lady of the family in which the deceased lived, had procured from a druggist some "drops" for toothache, and that the tincture was given her to use locally for that malady; but as it was found to produce its peculiar tingling and much unpleasant sensation in the mouth, its further use was abandoned. The bottle containing it had been put aside in a dark corner of a shutter-case, and being there left it had become forgotten by the members of the family. On the day preceding the suicide, the room had been cleaned out by the servants, and the deceased then discovered the bottle, which was nearly full, and labelled "poison." The druggist had broken no law in selling "poison," provided the poison were not arsenic. The jury, therefore, returned no verdict of default on his part, although it is certain that druggists place themselves, morally, in a very questionable position by selling deleterious substances to any one soever who may choose to ask for them. It is greatly to be regretted that some other violent poisons, such as corrosive sublimate, oxalic and prussic acids, etc., are not placed by the Legislature in the same category with arsenic, and certain penal restrictions imposed upon their indiscriminate sale. That the Act prohibiting the sale of arsenic, except under certain conditions, has been eminently effectual in preventing deaths from that poison, may be readily inferred from the fact, that during the last three years, only one death from arsenic has formed the subject of an inquest in the western division of Middlesex, and that instance occurred in a woman of unsound mind, whose husband used arsenic for the purpose of preserving stuffed animals. — *London Lancet.*

ON THE USE OF SALT-WATER BATHS IN CHOLERA. — *By Thomas Henry Starr, M. D., Edinburgh.* — Having observed some interesting remarks in the *Lancet*, respecting the virtues of salt as a prophylactic to epidemic cholera, and as the views there published in connection with that important subject coincide, in a great measure, with my own, I am induced to carry on the argument by furnishing the results of my recent experience thereon, as being indicative of the power and property possessed by salt, not only of warding off the assaults of the pestilence, but likewise, when properly applied, of preventing the fatal effects after it has seized upon the human body.

I should have tendered this communication for publication at an earlier period, had I not been led to transmit my statements to the General Board of Health in October last, in compliance with the requisition which was individually addressed to the profession a short time before, urging them to furnish special information on the disease wherever it was in their power so to do.

I have no doubt it will be allowed that I had reason to feel surprise when, upon examining the Report of the Treatment Committee of the Medical Council, and the classification of the two thousand seven hundred and forty-nine cases therein contained, I could find no reference whatever made to my contribution, or to any therapeutic method bearing the least resemblance to that which I had adopted with such signal advantage. I did not fail to remonstrate in the proper quarter; but



hitherto, the omission remains without any satisfactory explanation. Granting that my whole course of reasoning on the matter might emanate from too humble a source to be deemed worthy of official consideration, still I might claim a fair representation of the *facts* as they occurred to me, with a concise statement of the principles which guided me in the employment of a *remedy which proved so eminently successful*.

As it is generally admitted that the treatment of Asiatic cholera in the stage of collapse is a *questio vexata*, still open to inquiry, any practical information which tends to bring that fearful malady under subjection, must needs be regarded amongst the most important and valuable contributions to medical science.

As long since as the year 1849, I had reasons for entertaining the opinion, which I made known at the time, that the systematic and persevering use of the strong salt-water bath, at a specific temperature, ranging from one hundred and six degrees Fahr., to one hundred and twelve degrees Fahr., (the latter being the degree which comes under the denomination of fever heat,) would be found an expedient of uniform and great practical value in the worst forms of collapse, and more especially so when, from our earliest attendance, we find we have to deal with the disease after it has reached the point at which the intolerance of the stomach to the normal action of internal remedies renders the administration futile or even worse than useless. The course of reasoning which drew me to this conviction was founded, to say nothing of the laws of endosmose, on the vital property residing in the skin, of permitting the absorption of fluids into the circulating vessels, as proved by physiological experiments of admitted authenticity, as well as by the many familiar examples of the endemic transmissions of therapeutic agents when partially applied. It has been ascertained, moreover, that nutriment may be conveyed through the same channel; and Mr. Erasmus Wilson, in his valuable work on the Skin, says, at page 45, that "instances are on record, in which bathing in warm milk has been successfully employed as a means of supporting life, when the communication between the mouth and the stomach was impervious."

The remedial measure which I have tested and found so practically efficacious has the advantage of not requiring internal administration. Indeed, I am convinced that those practioners who exclusively or chiefly rely upon internal treatment in the collapse of cholera, will ever be doomed to disappointment in the great majority of such cases, for this obvious reason, that the stomach rejects them *in limine*; and as the blood, from the impetus of the disease, has already parted with the greater portion of its saline and watery constituents, whilst the residue has become cold and stagnant in the heart and vascular system, it stands to reason that our best chance for the reaction and recovery depends upon a prompt, vigorous, and well sustained restoration of its heat, its saline impregnation, and its purity. Without further comment, I will adduce the evidence which has confirmed my belief, that a favorable result may be almost uniformly accomplished under the worst symptoms of collapse by the comparatively simple and easy process which I have tried and recommend. The portion of salt I use is about half a pound to each gallon of water.

The sectional outbreak of the pestilence which supplied me with material for this communication, occurred here on Sunday, the 30th September, 1854. Its invasions and phenomena were characterized by a peculiar intensity and virulence; its distribution was circumscribed, whilst its existence was transient in the locality where it took place. The houses in which it appeared—viz: Nos. 2, 3, and 21—were crowded with inmates, and probably the least cleanly in the whole street, which is a comparatively obscure one, intersected by the boundary which separates the two parishes of Brighton and Hove.

Two children living on the first floor of 21 Cross-street, respectively named Wm. Moses Glover, aged nine, and John Glover, aged four, were suddenly seized with the epidemic about one o'clock, p. m., on Saturday, Sept., 30th, 1854. They both died shortly after midnight, little or no medical aid having been obtained for them.

The next case happened the following day—viz: Sunday, Oct. 1st. The name of the victim was Eliza Stevenson, aged fourteen. She was attacked, her mother told me, shortly before seven A. M., and, although she received the assiduous at-

tention of the medical officer of the district, she died the same evening, after twelve hours of great suffering.

The fourth case in this neighborhood was the first which came under my control. The patient's name was Sophia Pyrke, aged twenty-two, a servant out of place, lodging with her sister at No. 2 Cross-street. I was called in to this person on Monday, October 2nd, 1854, near midnight. She had been suddenly attacked with symptoms of the disease less than two hours before. When I saw her the cramps were very severe in the abdomen and lower limbs, the countenance was sunk and anxious; there was a remarkable coldness of the body, including the tongue and fauces, with frequent paroxysms of vomiting and purging of fluids, closely resembling rice-water; the pulse was scarcely perceptible. Other remedies, inclusive of calomel and cordial antispasmodics, failing, I had recourse to immersion in hot salt-water at one hundred degrees Fahr. Under its influence the action of the heart and pulse speedily rallied, and with the return of animal heat, the other dangerous symptoms gradually abated. The fever of reaction was slight, and this patient recovered rapidly.

The second case I attended was that of Thos. Buckwell, aged forty-three, living on the ground-floor of 21 Cross-street, being the same house where the children named Glover, died three days previously. He was attacked with the worst symptoms of the disease on the forenoon of Tuesday, October 3rd, 1854. I gave calomel freely, combined with opiate confection; I likewise gave cordials of various kinds; nevertheless, the characteristic vomiting and purging, with cramps, increased in severity until the evening, when he was completely exhausted, collapsed, cold, livid, and pulseless. Under these apparently hopeless circumstances, I resorted to prolonged immersion of the whole body in the hot salt-water bath, at one hundred degrees Fahr., with immediate, signal, and triumphant success. The pulse rose gradually under its influence; both sight and hearing, which had been much impaired, were simultaneously restored: the vomiting and purging became less frequent: the cramps left him, and the dejections soon presented a bilious tinge. The consecutive fever was considerable, and accompanied with a brownish tongue, and slight delirium. In a few days, however, under careful management, this patient became convalescent.

The third case on my list, from its extreme virulence and obstinacy, afforded, if possible, still more conclusive evidence as to the specific value of my mode of treatment. The sufferer's name was Ann Shearing, aged twenty-nine; married, and living in the top story of the house inhabited by the Glovers and Buckwell — viz: 21, Cross-street. She was seized with general prostration, vomiting, and purging, with violent cramps, on Tuesday evening, October 3rd, 1854. She was attended throughout the night by the medical officer of the district, who sent for me between six and seven o'clock the following morning, when I found the patient in a state of perfect collapse. Her countenance was shrunk, and death-like in the last degree: she was livid and without the least perceptible pulse: the heart, limbs, and abdomen were tormented by cramps, which came on in paroxysms, as did the retchings, accompanied with copious rice-water evacuations. She complained in a marked manner, but feeble, whining voice, of inability to see or hear distinctly. In this case, calomel had been administered in one large, and subsequent small doses, without any palpable benefit. In this unpromising condition she was plunged, with all possible dispatch, in a strong salt-water bath, which I steadily maintained at one hundred and ten degrees Fahr. Under its influence, the cramps almost instantly vanished. The heart's action became gradually excited, and the pulse at the wrist returned. I watch the rise and progress of the returning circulation and functions with intense interest and satisfaction; and with a succession of facts like those I have recounted staring me in the face, I could draw no other conclusion than that the brine bath, approaching fever heat, is a practical remedy which more effectually mitigates the sufferings, and approaches more nearly to the character of a specific, in the collapse of cholera, than any other with which I am acquainted; and I believe I have investigated the merits of them all. After being in the bath for half an hour, the patient was lifted out, and laid between hot blankets. The vomiting and purging had sensibly diminished in violence and frequency, whilst the dejections soon showed a bilious discoloration.

The arterial action and general warmth of the frame seemed to be well established. This improvement, with slight fluctuations, continued till the following morning, when the patient showed signs of relapsing into the worst stage of the disease. Without hesitation, I again had recourse to immersion in the hot salt-water bath, the curative effects of which were even more conspicuous and decisive than at first, inasmuch that the symptoms and danger were effectually and permanently subdued by it. The febrile reaction which ensued, though protracted for several days, was of a moderate kind, and by the expiration of a week, this patient was quite convalescent.

I might add to the foregoing cases, that of Elizabeth Glover, aged twenty-six, mother of the two children who first died. She was affected with the worst symptoms of the epidemic, and became my patient. She suffered very severely, and her recovery was mainly due to the hot salt-water treatment.

During the same week, there were two elderly people, named Martin, carried off by the Asiatic cholera, which ran its course very rapidly. They occupied the top floor of one of the infected houses—namely, No. 3 Cross-street. I was not concerned in the management of those cases, and I only adduce them as additional and undeniable proof of the malignant nature of the disease as it appeared in that locality.

I might strengthen the testimony in favor of my remedy, by adducing the case of an elderly woman, named Richardson, whom I attended a fortnight before in Upper North-street, Brighton; suffice it to say, at my first visit I found her rapidly advancing in the collapse of cholera. Her cure was brought about principally by the external agency of strong salt-water at a very high temperature.

The internal remedy (if so it may be called,) that I found most useful in the collapse, was Wenham lake ice. My patients said it materially assuaged their sufferings, and strange to relate “warmed” them. Its *modus operandi* seems to me to depend upon the astringent or constrictive property of cold, by reason of which it restrains the morbid elimination of serum from the gastro-intestinal mucous surfaces. I then used it simultaneously with the hot salt bath, the good effects of which it appeared to promote.

With regard to treatment in the premonitory stage, that is, anterior to collapse, I may observe that I arrested the disease at its onset in three, if not four, cases which occurred in the infected quarter, by the administration, not of tartar emetic or ipecacuanha, but by stimulant emetics, consisting of salt and mustard in equal proportions, followed by an active dose of calomel.

Lastly, it is worthy of notice, that the ravages of the pestilence were confined to three houses in Cross-street. Whether this interesting result depended on the disinfecting measures that were adopted, I cannot positively determine, but I am inclined to believe it was so, as I effectually fumigated them from the basement to the roof with chlorine gas, copious volumes of which I obtained by pouring one part of strong vitriol on a mixture of two parts of black oxide of manganese, four of salt, and one of water.—*London Lancet*.

LECTURE ON PNEUMONIA. — In No. 32 is a young girl, who has suffered from pneumonia for six days. The disease commenced in the ordinary manner, with shivering, vomiting, pain in the side, fever, and characteristic expectoration. On examination, a very extensive pneumonia was detected, occupying the apex of the left lung, descending in front and laterally to almost its base, leaving only the lower and posterior part of the organ in a sound state. The pulse was very frequent, and of very considerable force and fullness, when compared with the delicate constitution of the patient. To these symptoms were added labored respiration, extreme anxiety, and violent præcordial pain. These last symptoms led M. Trousseau to ask himself whether the pericardium had not become the seat of inflammation; for it is by no means rare to find a phlegmasia passing from the lung to the heart, and reciprocally, in consequence of the numerous anatomico-physiological ties that unite the two viscera, the coincident pericarditis being perhaps overlooked, owing to attention being exclusively fixed on the affection of the organ of respiration.



As in this patient, the entire absence of all physical signs proved the non-existence of pericarditis, the præcordial pain must be considered as depending upon intercostal neuritis, shown by M. Beau to prevail in inflammation of the parietal pleura—a neuritis which, although affecting the trunk of the nerve, produces pain at its periphery, where the nervous filaments terminate. Thus, a phlegmasia, affecting a portion of the pleura near the vertebral column, induces pain at the anterior part of the chest. So, too, in a pleurisy of the base we find the pain in the hypochondrium, and in that of the apex it has its seat, by reason of the obliquity of the intercostal nerves, in the præcordial region. This was the case with the patient, in whom the pneumonia was accompanied by a pleurisy of the apex.

The prognosis was serious for several reasons, such as the seat of the affection, its extent, and the violence of the febrile reaction. Bleeding seemed contra-indicated by the constitutional debility of the patient, and it was deemed determined to depend upon *kermes mineral*, a preparation of antimony of powerful action, far more manageable than tartar emetic, while it possesses nearly all its physiological and therapeutical attributes, causing like it vomiting, and at an early stage of its administration, and, somewhat later, diarrhœa. It is very much preferable to administer antimony in the pilular form. When it has been given for several days in succession in the fluid form, it produces in the throat and œsophagus a diffused, special irritation, followed by pustules, analogous to those brought out on the skin by the use of the ointment. *Kermes mineral* produces this effect as well as tartar emetic, though less speedily, and in a less degree. Laennec was aware of the existence of these pustules, and regarded them as a sign of antimonial saturation, analogous to mercurial saturation. That this was an error, is shown by giving the antimony in the form of pills, when pustulation never occurs. Another advantage they possess is that, even when given in double or triple doses, they will not so easily induce vomiting or diarrhœa as does the fluid. Thus, this patient took, during the first two days, fifteen grains, divided into ten pills, one every hour, without any inconvenience.

Some have attributed the therapeutical action of antimonials to an intestinal revulsion. Such was the opinion of Broussais, declaring that all Laennec did was to apply a blister to the intestines: and that admirable clinical observer, Chomel, entertained similar views. Laennec, on the other hand, believed that the remedy was absorbed, and accorded to it the contra-stimulant virtue claimed for it by the school of Rasori. We believe we may simplify the problem by suppressing the vomiting and purging, which furnish, in appearance, some grounds to the partisans of the revulsion theory. Thus, in this patient, with the most complete tolerance of the remedy, the pneumonia remaining locally the same, you find the heat of skin has disappeared, and there is scarcely any frequency of pulse remaining. The antimony has vanquished the reactional phenomena. Absorbed, it has acted upon the nervous system, and through this upon the centre of circulation. In proceeding thus we interrupt the chain established, through the intermedium of the nerves, between the circulatory centres and the organ affected, so that this last remains with its lesion isolated in the chest, as you may ascertain. But the fever and the general phenomena that accompanied it have disappeared, and this is an immense result: for, while we must not neglect the local condition, it is far more important to note the indirect action which this condition exerts upon the economy. It was against this action we directed our contra-stimulant, and we have been able to dissipate the excitement, the kind of titillation of the nervous system produced by the local lesion which remains now alone, deprived of the reactions which at first accompanied it. We must, in medicine, bear in mind other things than local conditions and suffering organs, regarding man as a whole, and the manner in which he is impressed by these. It is to the general condition alone that medication is oftentimes addressed, and that even by the partisans of localization, who are far less exclusive than they believe, boldly opening a vein in pneumonia without considering whether this practice is in accordance with their theoretical views.

The *kermes* will in this case be continued for some days and then suspended, or we shall find the very pustulation produced in the stomach and intestines that we sought to avoid in the upper part of the canal. In a patient in one of the wards



convalescence has been delayed by an enteritis thus artificially induced. As a general rule, medication is continued too long in interal affections, perhaps because we are not able to form an exact idea of the conditions of the parts. We should rather imitate the conduct of the surgeon, who after having procured a free issue for the pus of a phlegmon, leaves to nature the task of resolving the peripheric engorgement and remaining inflammation. The proper time for staying the hand, is when resolution has freely commenced. Thus, in pneumonia, as soon as the general phenomena have disappeared, and we hear the local signs of returning normal respiration, we must suspend medicines and have recourse to food.

*Rheumatic Pneumonia.* — About a month since, a young man was admitted with all the signs of pneumonia, and kermes having been administered, the next day all traces of the pneumonia had disappeared. To what were we to attribute so sudden a retrocession? Was it the result of treatment, or must we seek for the cause in some peculiarities attaching to the nature of the disease itself? The latter interpretation received some light from what was observed at the next visit, when the left great toe was found red, swollen, and painful, the tendinous sheaths along the dorsum of the foot exhibited a like condition. Next day, the right foot was similarly affected, though in a less degree. Two days ago, a woman was admitted with the following symptoms: strong febrile action, redness and swelling of the left leg and foot, and severe pain in the entire upper extremity and trunk of the same side, the pain exciting cries on moving the parts. The patient especially suffered at the left side of the chest, but no abnormal sounds were audible. During the night, cough came on, and in the morning a manifest *souffle* was audible in the supra-spinal fossa, while around and in the infra-spinal fossa was heard a fine sub-crepitant *râle*. During the cough, dry crepitating *râle* and bronchophony were heard, and two or three pneumonic sputa were expelled. This morning all signs of pneumonia have vanished. Here again I hesitate to attribute such prompt resolution to the treatment, especially as the apex was the part involved — a form of pneumonia regarded by all physicians as especially serious. I prefer explaining so rapid a termination by the nature of the pneumonia itself, which I regard as *rheumatic*.

Too partial to localization, practitioners are only accustomed to recognize rheumatism as affecting certain tissues, viz: the muscles, the aponeuroses, and the joints, and when it manifests itself elsewhere they call it by some other name. This is as if we only acknowledge syphilis as we observe it on the penis, and made so many distinct affections of its manifestations on the throat, skin, etc. But syphilis is recognized to be the disease in all these accidents, and why should it not be so with rheumatism? That it attacks all serous membranes is an indisputable fact since Bouillaud's beautiful researches, which have so much advanced the pathology of the heart. When in the course of acute articular rheumatism any of the serous membranes become affected, it is termed a pericarditis, pleuritis, meningitis, etc., according to the membrane attacked. This is right enough as far as it goes; but for the proper denomination of the disease, which is a kind of definition in a single word, we ought to add the epithet "rheumatic." When a man accustomed to suffer from rheumatism acquires, as a consequence of cold, a pain of the shoulder, hip, etc., he at once says he has an attack of rheumatism. But instead of this pain let there be a soar throat, and both patient and doctor cease to be logical, and call it angina instead of rheumatism; just as if there were not a true rheumatic pain of the fibrous parts of the pharynx and palate, pain followed by fluxion, tumefaction, and redness of the pharyngeal mucous membrane. Do we not find rheumatism of the fibro-serous tissues of a joint accompanied by tumefaction of the subcutaneous cellular tissue, and bright redness of the skin; and why should we not admit the same influence in the delicate and vascular mucous membrane? For my part, I should not hesitate to recognize a rheumatism in such a case, or, if you like it better, a rheumatic angina.

This distinction may serve for the explanation of the very great differences observed in the progress and termination of anginas, regarded by some physicians as being of the same nature. Thus, simple inflammation of the tonsils goes through all its stages, in spite of whatever treatment may be opposed to it, and a patient accustomed to such attacks will warn his attendants of the inutility of

endeavoring to prevent the formation of abscess. A rheumatic angina, on the contrary, will often disappear in the course of a night, whatever the treatment adopted, leaving the physician astonished at his therapeutical success, the result, however, being really due to the essentially mobile character of the affection. Descending lower down in the digestive canal, we can explain those sudden diarrhœas which manifest themselves under the influence of a chill. The fibrous portions of the canal become painful, and the contractions more considerable and more frequent, a fluxionary movement being at the same time established towards the mucous membrane, the secretions of which are increased. Such diarrhœas are of short duration, unless, indeed, the rheumatism takes on, as it may anywhere, a chronic character.

After these considerations, does it seem strange to admit a rheumatic pneumonia? Suppose the pulmonary tissue, or, what is the same thing, the fibrous tissue of the extremity of the bronchi, becomes seized with rheumatism, what are the immediate results? Tumefaction and congestion of the mucous membrane and an infiltration of the cellular tissue; that is to say, the anatomico-pathological conditions of œdema or of pneumonia in its earliest stage; with this peculiarity, that such lesions, participating in the fugacious nature of rheumatism, do not possess the fixity and persistence of the lesions of ordinary pneumonia. It is in cases like these that therapeutical results seem so marvelous, and so they would in our own two cases had we not a better reason to give for the rapidity of the cure. They were in fact, the examples of rheumatic pneumonia, the one occurring in a young man who was at the same time suffering from rheumatism of both feet, and the other in a girl who had formerly had rheumatism, and together with the pain in the chest complained of rheumatic pains along the whole of the same side of the body. In similar cases, I shall not hesitate to admit the existence of rheumatic pneumonia, too happy only thus to complete my diagnosis, and to become enlightened as to the amount of importance that should properly attach to my therapeutics — *London Lancet*.

CASE OF EARLY MENSTRUATION.—By J. O. Bronson, M. D.—To the few authentic recorded cases of extraordinary precocious physical development, must be added the following, which those present had the pleasure of seeing at Prof. Barker's clinic for diseases of women and children, on the 27th of July, and again on the 14th of August. It is a case of early menstruation, the subject of which is named Phœbe Ann Baker, now four years and seven months old, having been born on the 19th of January, 1851, in Sing Sing, Westchester county, N. Y. At the age of ten months her menses appeared, accompanied by the usual signs and developments, and have continued with healthy regularity ever since.

The girl is large for her age, with light brown hair and complexion, and blue eyes. Her form is mature. Her mammæ are prominent, the size of an orange; pelvis wide, and her pubis is covered with hair. In fact she is a woman in physical, and a child in her mental developments. She is quite unconscious of her condition, having no uncommon degree of modesty, and seem to have no sexual feelings. She is healthy and strong. The catamenial discharge is healthy in color, character and quantity. The performance of the function is not accompanied with pain. Nothing concerning the parents, or otherwise, was elicited, tending to throw light upon the causes of this early establishment of the female function. This case cannot be classed with those of disease simulating menstruation, but as a *bona fide* case of infantile puberty.

There are some interesting cases of precocious puberty, and even of parturition, on record of which I will mention some of the most remarkable.

A case was transmitted to the Medical Society of Toulouse, by M. Gaugiran, of some interest. In June 1810, he was called to see a young girl aged five years and three months, the subject of menstruation from three years, in whom the female developments were equal to the case above reported. M. Gaugiran was called to treat her for a suspension of the catamenial function. She was affected by the ordinary symptoms of chlorosis. Being subjected to treatment for that malady, the mensis appeared again, and her health was completely restored.

Mr. Ranoë details a case in the Transactions of the Royal Society of Copenhagen (t. iv. p. 44), of a girl who "put on womanhood" at the age of three years.

In the German Ephemerides (dec. 3, an. 2, p. 262), a case of precocious menstruation, pregnancy, and parturition at the age of nine years, is published.

M. Schmith, in Sue's *Essais Historiques*, Paris, 1779 (t. ii. p. 344), gives an account of a girl who became the subject of menstruation at the age of two years, and at the age of eight years gave birth to a dead child, at the full period of gestation.

Sir Astley Cooper gives the history of a case of early menstruation, reported in the *Medico-chirurgical Transactions* (vol. iv. p. 490), which is interesting more on account of the minuteness of details, than the peculiarity of the case. The catamenia appeared at the age of three years.

M. Brierre de Boismont, in his work *De la Menstruation, &c.*, (p. 32), gives some interesting instances, which, however, not being of earlier development than the above cases, I will not quote. He mentions cases, but does not detail them, referring to Haller, in his *Element Physiologica*, (t. viii.), where menstruation appeared as early as six and even three months. I have not been able to obtain the work, and therefore can do no more than M. Brierre de Boismont.

In the *Hist. de l'Acad. Roy. des Sciences*, for 1761, p. 59, a most interesting case is reported, of a female child which bore at her birth all the external marks of puberty. The menses appeared at the age of four months, and had continued to the time of the record which was four years.

This is the only instance to which I can refer where menstruation appeared earlier than in the subject of this notice; and among the recorded cases, but few are found to possess the regularity and health of the above case.

CHEMICAL EXAMINATION OF THE AIR CONTAINED IN BUOYANT LUNGS IN CASES OF SUSPECTED INFANTICIDE.—*By Henry Hough Watson, Esq.*—It has long been matter of surprise to me, on reading reports of suspected infanticide, that medical men, in examining the state of the lungs, frequently, if not generally, resort to the hydrostatic test, almost *per se*; and that from the floating or non-floating in water, they conclude that a child has or has not breathed. I cannot but conceive that, unless, in addition to the usual hydrostatic test, a chemical examination be also made of the air contained in the lungs, when this organ is found to float, and particularly when even the slightest indications of putrefaction or decomposition are perceptible, that full and sufficient investigation has not been made, which ought to be required, before an opinion should be relied upon, that the buoyancy is owing to inflation with atmospheric air, or with the products of breathed atmospheric air. No member of the Medical Profession is ignorant of the facts, that atmospheric air, and the products of breathing it, are not the only air capable of giving buoyancy, and that other kinds of air are liable to inflate the lungs in a greater or less degree, according as decomposition has advanced much or little; and it might be thought that persons, not ignorant of those facts, could not, without hesitation, make a decided assertion of their opinion, that a child has been born alive, when the principal evidence is the floating of the lungs in water, and when an examination of the quality of the air contained in the lungs has not been made; yet we sometimes do find medical men stating that they consider the fact of the lungs (or part of the lungs) floating in water to be conclusive proof that the child has breathed; and while admitting that the lungs would float from decomposition, still at the same time saying that, in the instance in question, decomposition has not proceeded "far enough" to be the cause—there is not "sufficient decomposition" for the lungs to float in water from that cause, and they have "not the slightest doubt but the child has breathed."

It is admitted that, in some instances, only a very small quantity of air is sufficient to cause lungs to float; but I am not aware that it has been satisfactorily determined how little decomposition may be sufficient to produce that small quantity of air. I believe, however, that it has at least been intimated, that air from putrefaction may be formed in lungs, without any change in color, smell, or other properties of the lungs being observable. Why, then, not resort to a chemical examination of the air, the cause of the floating of the lungs, before a decided



opinion is formed of this cause? Comparatively few medical men are practically acquainted with chemistry, and probably this may be the chief reason why the examinations in cases of supposed infanticide are often thus defective: still, where there is a risk of innocence being charged with crime, this consideration should not allow superficial examination to be accepted, instead of searching inquiry, carried to the utmost limit to which the power of science can assist. To a chemical mind, after the question, Do the lungs float? and an answer in the affirmative, the next required to be answered must be what is the character, quality, or composition of the air contained in the lungs? and nothing short of an attempt to ascertain the fact can satisfy. Few, if any, authors on forensic medicine, whose works are usually consulted, give instructions for examination on this head: neither do they suggest that the examination be made. But, without the examination having been made, it seems to me quite as unreasonable to conclude that a child has been born alive or has breathed, because its lungs or a portion of them float in water, as it would be to assert that one has been born dead, or has not breathed, merely because its lungs sink in water. In two suspicious cases which were submitted to me, and in which the lungs floated, I carried out the investigation: and in both instances the result was that, decomposition having taken place, the air I obtained from the lungs contained combustible gas, as proved by firing it by an electric spark when mixed with oxygen in a Volta's eudiometer. I cannot but think it would be well if a similar examination was always resorted to: particularly in cases where putrefaction is in a slight degree perceptible: in which case it might be that the examination would give rise to doubt in the mind of the operator as to whether the buoyancy was due to respiration, while, by neglect of the examination, he might, perhaps, too boldly conclude that respiration had taken place: be thereby being the means of an innocent person being accused of guilt. If the operator found the presence of combustible gas, he might say, I have tested the air which caused the lungs to float, and have found combustible gas in it (hydrogen, carburated hydrogen, or carbonic oxide, as the case might be): and this not being a constituent of atmospheric air, nor a product of respiration, but a product of organic decomposition, it follows that decomposition of the body has commenced, and it is impossible to conclude otherwise than that the floating of the lungs may be owing to decomposition wholly or in part; that there is no proof that atmospheric air has been inspired: on the contrary, if he found no combustible gas, but indications of oxygen, carbonic acid and nitrogen, he might say, I have tested the air, and have found it to consist of such gases as may be the result only of atmospheric air having been inspired. It certainly will be much more agreeable to a conscientious man to have facts such as these to base his opinion upon, rather than the restricted and doubtful evidence afforded by the hydrostatic test simply, and the hazardous conjecture as to whether decomposition has proceeded "far enough" to give buoyancy to the lungs. The experiments required in making the investigation are not difficult, and, in skilful hands, they may be performed on a small quantity of air. To collect the air, it is only necessary to immerse the lungs in a vessel of water, and under a funnel the neck of which is inserted in the neck of an inverted vial filled with water, and also immersed; then to puncture the lungs in several places, and to squeeze them when the air will ascend into the vial, which, with the funnel, should be carefully held by an assistant. The operator should be careful that no small bubbles of air be adhering to his hands or to the outside of the lungs. &c., in the water before he begins to squeeze, lest such ascend into the bottle. 100 or 200, or even 50, water grain measures of the air of the lungs will be sufficient, if no more can readily be obtained; and when so much has been received into the vial, it should be transferred to a graduated eudiometer (of about three-tenths of an inch in diameter) and the quantity measured. I first fill the eudiometer with clear lime-water, and then pass the air up into it from the vial, noting any milkiness which may be formed owing to the presence of carbonic acid, and which is likely to be formed whether the air be that of putrefaction or the result of breathing. I continue to wash with lime-water in the usual way till all the carbonic acid is removed; and then I divide the remaining air into two or more portions; to one of which I add about a third of its bulk of nitrous gas (deutoxide of nitrogen), and if any yellow fume and diminution of volume are produced, the presence of oxygen is indicated, the quantity of which I calculate. To the residue



I add oxygen, 90 or more per cent. pure (the degree of purity having been previously ascertained by firing it with hydrogen), and so much of it as not only to take out the whole of nitrous gas, but as to add about one-third to the residue thereafter. The whole is then transferred in the Volta's eudiometer, and an electric spark passed: if explosion takes place, the residue is transferred back into the other eudiometer, filled with lime-water; any milkiness, indicating that carbonic acid has been formed by the explosion, being noted. In another experiment I take one of the other portions of the air left after washing with lime-water, in the first instance; add to it about one-third of its bulk of the oxygen gas, pass the spark, and note whether there be explosion; and, if there be explosion, pass again through lime-water as before, to detect any carbonic acid which may have been formed.

From the above it will be perceived that several experiments may be made on a very small quantity of air. It is advisable, however, that the water used should be rain or soft-water, and that it should have been boiled, and allowed to grow cold, while excluded from the atmosphere in corked bottles or the like, so as to prevent as much as possible the air in question from becoming mixed with other air from the water.

I do not recommend this testing to the exclusion of all or any of the other various means and circumstances from which opinions are sometimes formed. I advise that it be had recourse to as a useful auxiliary to the hydrostatic test, in directing the answer to that often very difficult question, Has the child been born alive, or has it ever even breathed?—*London Lancet*.

**CHLOROFORMING AN ELEPHANT.**—The large elephant, 120 years of age, late the property of Mr. Wombwell, being incapable of locomotion from diseased feet, the present proprietor gave directions for its destruction in the easiest manner possible. Accordingly, Mr. E. Price, veterinary surgeon, and Mr. Flewitt, chemist, of Birmingham, on Friday last proceeded to carry his wishes into effect. In ten minutes from the application of chloroform, the animal became totally insensible; prussic acid was then administered without effect, and two large doses of strychnine, without producing any perceptible feeling of pain. Not succeeding in these endeavors, a continuous application of chloroform was made, in the hope that the animal might sink under it, but, after three hours' perseverance in this course, no difference of respiration being observed, the administration was stopped, and in one hour and a-half all effects of it had passed away. The only known means of destruction left were the knife and the bullet. The latter was first tried (the animal being again chloroformed) and wounded a branch of the carotid artery, from which blood flowed in a tolerably sized jet. The opening was then enlarged by a knife, and the artery fully divided. In a few minutes the huge animal ceased to live, having died without a struggle or evidencing the slightest feeling of pain.—*London Lancet*.

**WOUND OF THE POSTERIOR TIBIAL ARTERY SUCCESSFULLY TREATED BY PRESSURE AT THE SITE OF INJURY, AND COMPRESSION OF THE POPLITEAL AND FEMORAL ARTERIES; COMPLICATION; TRAUMATIC DELIRIUM; RECOVERY.**—On the 6th of March, 1854, at half past one, A. M., Thomas Ryan, æt. 28, a police constable, was brought to Mercer's Hospital, having sustained several wounds accompanied by severe loss of blood. These had been inflicted by a knife in a struggle with a prisoner. The most serious wound was in the calf of the left leg, at the junction of the middle with the upper third. A large amount of blood had been lost, and when brought to the hospital the bleeding broke out afresh. From the position of the wound and extent of the hemorrhage, there could be no doubt that the posterior tibial artery was injured. The wound was about three quarters of an inch in extent, and parallel to the long axis of the limb. The patient had fainted before being seen by Mr. Butcher, and had all the symptoms of excessive loss of blood.

Pressure was applied by means of graduated compresses over the wounds in the calf, and a rolled bandage as a pad over the popliteal artery. The limb was extended on an inclined plane, and the foot raised. In the course of about an hour reaction was fairly established.

On the 7th March, as the patient complained of uneasiness from the pad in the ham, an aneurism compressor in the groin was substituted.

On the evening of 8th March he was seized with delirium, which lasted about three days.

On 12th March the compresses were removed, and simple dressing substituted, compression of the femoral, however, being kept up till the 27th, when it was abandoned.

On 15th April the patient was dismissed cured.

The good effects of not seeking by operation for the wounded artery, unless bleeding was actually going on, was exemplified in this case, for pressure answered all the indications for accomplishing a cure. The advantages also of employing the aneurism compressor deserve to be pointed out, as by this means the current of blood circulating in the main artery is controlled, and time is given to the wounded vessel for the permanent sealing of its divided coats.—*Dublin Quarterly Journal of Medical Science.*

**WOUND OF THE ULNAR ARTERY ABOVE THE WRIST SUCCESSFULLY TREATED BY COMPRESSION AT THE WOUND, AND PFESSION OVER THE BRACHIAL ARTERY.**—James Nicol, æt. 43, a shoemaker, was admitted into Mercer's Hospital on the 22d March, 1854. He was accidentally wounded by a knife above the wrist, which inflicted a transverse wound in front about an inch in extent, severing two of the flexor tendons and the ulnar artery. Bleeding was violent, and the man almost instantly fainted. On examining the wound only a small superficial artery required ligation, but it was evident from the position of the wound and the great loss of blood, that the ulnar had also been divided. Compresses were applied to the wound and to the brachial artery.

On the 31st the compress was cast off by suppuration.

No further bleeding took place, and the patient was dismissed 6th April.

The successful issue of these cases shows the propriety of not looking for the wounded vessel, unless actual hemorrhage is present; but wounds of arteries in the neck must be excepted, as the hemorrhage is too rapid and profuse, and the wounded vessel is too near the heart to admit of a coagulum forming capable of resisting the column of blood. In these cases, then, diligent search should be instituted, and the vessel secured above and below the wounded point, *when practicable*; if not, the common carotid should be tied.—*Ibid.*

**ANTISEPTIC PROPERTIES OF IODINE.**—M. Duroy, a druggist in Paris, submits to the Academy of Medicine certain experiments on iodine, which have been favorably received by that body, although it appears that M. Magendie had previously arrived, by less detailed experiments, at a similar conclusion. M. Duroy had his attention called to the antiseptic properties of iodine by a case under the care of MM. Piorry and Forget, in which this substance was injected into an abscess in connection with caries of the vertebræ. The ioduretted water injected was found on its removal, two hours afterwards, to have lost the color and odor of the iodine; and a test paper impregnated with starch showed that in fact all the free iodine had disappeared. Further experiments showed that this was owing to its entering into combination with the albuminous elements of the pus; and that a similar combination took place when blood, milk, white of egg, and gluten, were placed in contact with free iodine. Further, it appears that albuminous substances have so strong an affinity for iodine as to withdraw it from a previous combination with starch, and to discharge the blue color which is the well known result of this combination. (In the presence of organic substances, therefore, starch cannot be securely used as a test for iodine.) In a watery solution iodine does not, according to M. Duroy, coagulate or otherwise alter the form of the albuminous matters with which it combines, but it possesses the power of arresting putrefactive changes; and, after the lapse of a week, ioduretted milk, blood, albumen, and gluten, were found unchanged, while similar quantities, without any addition of iodine, had undergone notable decomposition.

M. Duroy ascribes some of the therapeutic powers of iodine to its antiseptic properties, and suggests that it might be possibly employed with a good result to neutralize certain organic infections and poisons, such as the miasmatic or ague-poison. The commission of the Academy remark, that the experiments of Brainard and Greene, performed since M. Duroy's researches, have tended to show the neutralization of the rattlesnake poison and of the curare (woorara) by iodine under the form of "extraite de lianes."—*Bulletin de l'Académie*, 15th Aug., 1854.

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Quarantine and Yellow Fever at Natchez

DURING 1853-'54 AND '55:

*The futility of the former and the local origin of the latter conclusively shown; and admitted for 1855, by the most strenuous advocates of the one, and the most ardent disbelievers of the other.*

*By C. H. STONE, M. D., of Natchez.*

To begin with the year 1855. Quarantine was established on the 19th July, and the station fixed four miles below the city. The health of the people continued good, with the exception to be stated; no deaths from disease having occurred for four or five weeks.

On the 4th September, the Board of Health published the following information for the people.

“TO THE BOARD OF HEALTH:

*Gentlemen:*—I have the pleasure of reporting our city remarkably healthy; we have had no infectious diseases, and fewer cases of the bilious remittent and intermittent fevers than I have ever known. There is no sickness at the quarantine station; I think the indications are clear that we shall have a very healthy season.

Yours, respectfully,

L. P. BLACKBURN.”

Natchez, September 3d, 1855.

On the 6th September, the following card, dated the 5th, was published.

“The undersigned, practicing physicians of Natchez, deem it their duty to state, that within a few weeks, several cases of yellow fever have occurred in this city; some of which have recovered, others have died, and at this time, there are other cases existing.

W. HARPER,  
W. L. JONES,  
C. H. STONE,  
JNO. C. INGE,  
H. LYLE.”



On the 8th September, the Board of Health met, and the following was the result of their labors.

CITY HALL, September 8th, 1855.

"At a called meeting of the Board of Health, held this day at 3 o'clock, P. M. : Present, R. W. Wood, Esq., Mayor; Dr. L. P. Blackburn, Health Officer; Messrs. Burns and Doniphan."

The following report of Dr. Blackburn was submitted, which, on motion of Mr. Doniphan, was adopted.

"TO THE BOARD OF HEALTH OF THE CITY OF NATCHEZ :

*Gentlemen* : — It becomes my painful duty to announce to you the existence of yellow fever in our city.

Yours, respectfully,

L. P. BLACKBURN, *Health Officer.*"

The following preamble and resolution of Mr. Doniphan was, on motion of Dr. Blackburn, unanimously adopted :

"The City Council, with commendable diligence, as soon as yellow fever was announced in New Orleans, established, and have continued to enforce rigidly the quarantine laws against all boats and their passengers from infected ports, and have also had an armed guard on the roads leading to the city, as well as a secret police officer to detect infractions of the law, and have done every thing in their power to keep Natchez free from exotic yellow fever; notwithstanding all which it is in our midst, and threatens to become epidemic,

"*Therefore Resolved*, That the yellow fever now existing among us, is a visitation of an all wise Providence, against which, the most judicious human efforts have been unavailing, and while we deplore the fact, we feel it our duty to make it known to our citizens, that they may take such measures to avoid the infected atmosphere as they may deem advisable.

*Resolved*, That the proceedings of this Board be published in hand bill form, and posted on the streets, and also inserted in the papers on Tuesday. On motion, the Board then adjourned.

W. H. STEWARD,

*Secretary of the Board of Health.*"

Now for the origin of this yellow fever :

*First Locality.* The first cases that came under my notice, were in the family of Mr. J. D. Shields — his wife, four children, and five servants, and the first no doubt in the city, as follows: August 17th, Bayard Shields, aged 6 years; 17th, Charles, servant, aged 8 years; 19th, Mary, servant, aged 11 years; 19th, Joe, servant, aged 50 years; 20th, Mrs. Shields; 20th, Dunbar Shields, aged 13 years; 21st, Barthena, servant, aged 14 years; 21st, Abijah Shields, aged 2 years; and on the 22d, Sye, servant, aged 25 years.



Before these cases occurred, I had once visited Wintz, a servant, 35 years, on the 8th August, then sick one or two days. She had had yellow fever in 1853, and I saw nothing peculiar in her attack this year. On the 12th August, Judy, 8 years, was taken sick, and on the 14th, Mary, 40 years, servant. I did not see these cases, but Mrs. Shields, who has had much experience in Southern fevers, assures me that the attacks of Wintz and Mary, were exactly like each other, and different from all the rest; and that Judy's was as much unlike these, as precisely similar to Bayard's and the others. The reason for thus stating these three cases, will be apparent in the sequel. Up to the 15th October, these persons have remained free from diseases.

It is well to state here, that Mrs. Dr. Chamberlain, and Mrs. Gab B. Shields, both living in the country, had attacks of yellow fever after visiting Mrs. Shields' residence; the former remaining there 36 hours, and the latter being there several times.

On the 22d August, a case occurred at Mr. J. Bradley's, adjoining Mr. Shields.

On the 23d August, Mr. Skinner, and on the 26th, Mr. Knickerbocker, (both residing at Mr. Walworth's, also adjoining Mr. Shields,) and on the 26th, Mr. Walworth's servant, Mark, had attacks. Mr. Walworth's family were absent, and his servants, excepting Mark and one other, were protected persons.

The residences of Shields, Bradley and Walworth, are on a high, dry and clear ridge, one third of a mile north east from Main street, or the centre of the city, and are the only one's on twenty or more acres of land.

*Second Locality.* On the 19th August, occurred the case of Kennedy, an Irishman, laboring in a brick yard, one third of a mile south-west from the city, and living in the Lancaster Row, on the second square south-west from the centre of the city, and one-third of a mile from the first locality. On the 25th, Kennedy's mother, living in the same house was attacked.

*Third Locality.* The first case under Dr. Harper was on the 20th August, Greeincher, near the Cotton Press, and near the river, and one mile north from the centre of the city, and half a mile north from the first locality.

*Fourth Locality.* The first cases under Dr. Lyle, were on the 21st August, George, a young man, living with Crone & Swarty on Main street, in the centre of the city; on the 24th August, Mrs. Staniforth, living three squares east from Main street, and centre of the city; on 27th August,

Boas and his wife, living one square from Main street, quarter of a mile south-west from centre of the city, and within two squares of Kennedy's, or the second locality, one-third of a mile from the first locality, also, a third of a mile from Mrs. Stansforth's, and one mile from the third locality.

No doubt, other cases occurred at or near these dates, and at other points of the city, I mention but one very probable one. It was called before death by Dr. Davis, "Typhus Stupidus," and was reported in the papers as dead on 31st August, of "Typhus" — "*Stupidus*" being left off, and *Icterodes* not being added.

The cases under Drs. Harper, Lyle and myself, had no communication with each other — with steamers or with goods, wares or merchandize from infected districts, of which, there was but one in the South at the time. But those at Mr. Shields', and the out break of the disease in the city, have been attributed by some to *one change of clothing* worn by Mr. S., while remaining forty-eight hours between the 30th July and 1st August, on the wharf boat at the quarantine station, where, at that time and before, there had not been a single case of the disease !!!

Let me see. Wintz took the soiled clothing from Mr. Shields' carpet bag, threw them in the sun, and they remained exposed in the open air all night.

Mary washed them the next day, August 2d. Wintz, who had had yellow fever in 1853 was *sick* on the 8th. Judy, who had not touched the clothes on the 12th, and Mary on the 14th. If Wintz and Mary, or Mary only, alone received the yellow fever from these clothes from touching them, so did Judy without doing so, and Mr. Shields' should have been the first case.

Whether Mr. Shields had any attack I am not disposed to assert or deny. He was suffering on the 21st and 22d August, in the manner that Dr. Lewis, of Mobile, describes as sometimes happening, and very much as Mr. Elliot, whose case is to be mentioned as one of Mr. Pearsall's attendants, and moreover, he has remained free from attack to the present writing, 15th October. If Wintz had a second attack, Mary, far more probably, had a first one. If Wintz and Mary had yellow fever, Judy, most certainly, had the same, which fixes the dates of the first cases, on the 6th or 7th of August for Wintz — on 12th for Judy; and on the 14th for Mary. But it is of small moment about these cases and dates.

The disease was in Mr. Shields' family most clearly on the 17th August, and the question is, was the poison or contagion of yellow fever conveyed to his house in a *two foot* carpet bag, containing one change of soiled

clothes, and a few clean ones, and again, how did this virulent contagion get into the carpet bag! As many people believe in this power of yellow fever contagion, now that their fear of the disease has been wrought to such tension, I propose to show that the thing is simply impossible. The evidence given must dispel any such idea from the minds of all, here or elsewhere.

The yellow fever was *said* to have been introduced into the Island of Boâ Vista, one of the Cape de Verdes, by the British Steamship *Eclair* in 1845. Dr. McWilliams was directed to inquire into it, and in his report makes the following statement: *Twelve bags* of soiled clothing were landed on the 21st August, and distributed to seventeen washerwomen on the next day. The testimony respecting the periods of attack of thirteen of them, was taken in April 1846, four having died, but of what disease or when taken sick, nothing is said. The names are omitted here, though given in the report.

No. 1. Had fever late in December.

No. 2. Had fever late in January.

No. 3. Was sick after No. 1.

No. 4. Had fever in January, her mother, father and two brothers were taken first.

No. 5. Not sick until lately, (near April, 1846.)

No. 6. Never had fever.

No. 7. Taken about 20th October.

No. 8. Does not know the exact time, but not till Mary had died.

No. 9. Had not the fever until after her husband, who was attacked after the fever became general.

No. 10. Never had the fever.

No. 11. Had fever about the same time with the rest of the family.

No. 12. Had fever after her brother, who died sometime in November.

No. 13. Was not attacked till January.

If such be the result, actually *nothing*, from washing, what may be reasonably calculated for seventeen women at a thousand, not to say one thousand seven hundred pieces, and these, from a ship in the tropics reeking with *African* yellow fever; how much *New Orleans* yellow fever ought to have been carried into the city of Natchez by *one change* in a two foot carpet bag, which remained two days on a wharfboat at the Natchez quarantine station, where there had not been one case of that disease? Certainly, *less than none*? But, perhaps, by certain shakes, up, down, horizontal, the potency of nothing was exalted to the thirtieth degree, and the spiritualized

attention received in infinitesimal doses sufficed to do the deed. An example of *much* from *nothing*, in which some people are found to believe. I hope my friend Mr. Shields, will in future shake his bag less potently, or perhaps better reverse his shakes.

1853. It has been said that the epidemic of 1853 was owing to the introduction of the case of Mr. Pearsall, who came from New Orleans ill of the disease, and died at the Mansion House, on the 17th July.

Dr. Blackburn and Dr. Davis make this statement, and I offer the following proof that it is not true; nor is it true that Murray, who came home on the day of quarantine ill of the disease, and died on the 26th July, caused the epidemic.

The first cases of that year, were as follows:

*The First* on the 14th July, a foreigner, who arrived in New Orleans on the 13th or 14th of June, where he remained four or five days, and reached this place on the 20th. He either contracted the disease here, or his case shows a period of incubation of twenty-six days.

*The Second* on the 15th; *third* on 26th; *fourth* on 25th; *fifth* on 27th; *sixth*, Larry Curtiss on 31st July or 1st August, a barkeeper in Mansion House, and one of Pearsall's visitors; the *seventh* on 2d August, whose arrival, stay and departure from New Orleans, and arrival here, were the same as the first case. The periods of incubation for her, will be forty-three days, or she received the disease here.

These cases, except the *first* and *seventh*, were far removed from each other, and all except Curtiss, were on different squares from the Mansion House.

A man, I believe a barkeeper, died at the Mansion on the 2d of August, from falling over the bannisters while in delirium from drink.

Dr. Lyle states, that his first case was on the 16th July, and from the 17th, "almost every case assumed that form."

I give the following account of the attendants and visitors of Mr. Pearsall, which I have every reason to believe constitutes the whole truth.

*Mr. Hugh Elliot* was more constantly with him than any other person, Gus excepted. About the middle of August, (about twenty-eight days from the 17th July,) Mr. E., was confined to his bed for four or five hours, felt badly for three or four days after, and has had no other sickness since, though exposed during the remainder of that and to the 15th October, of this year. The disease was very generally prevailing before his attack.

Gus belongs to Capt. Knight, who says that he has never had yellow fever.



There were two barbers to shave him, but of these I can learn nothing, as they are not in the city.

*Mr. J. R. Mitchell* did not have yellow fever till 1854, and then in New Orleans.

*Larry Curtiss*, who *Mr. Elliot* shows was in the room at the sealing of the coffin, but does not remember to have been there at other times, was attacked on July 31st or August 1st.

*Dr. L. P. Blackburn* was his attending physician, and does not admit that he has ever had yellow fever.

I have been unable to learn that other persons were in his room, except *Dr. Lyle*.

If the preceding evidence be not sufficient to show that *Pearsall's* case had no agency in the production of the epidemic, of which, cases began *before* his, I am thrown under the necessity of denying, almost that we had any epidemic at all, certainly not till thirty or forty days after his death, and then to have begun in the usual mode; cases here and there, become more and more numerous till prevailing generally. This has usually taken several weeks, and would carry us into the middle or last week of October. At this date, the epidemic of 1855 is nearly at an end, perhaps only for want of subjects. And I prove this position in this wise. After the announcement by the city authorities as advised by me, then Health Officer, that an epidemic was impending over us, (for which, I was bitterly abused for a week,) *Dr. Blackburn* for about two weeks, and *Dr. Davis* for three or four days, denied that there was any yellow fever in the city; forty days, (quarantine number,) at least having elapsed since *Pearsall's* case according to the former, and thirty or more days according to the latter authority, and no disease resulting till the middle of October—that is, provided these two authorities were not mistaken in 1853, as they were in 1848 and in 1855. I think it more than the people care to believe, or have bargained for, that one or two cases can cause an epidemic thirty or forty days after introduction. This is like running ahead of the fox.

In 1854, *Dr. Blackburn* being Health Officer, quarantine was established on the 25th August, long after yellow fever was known to exist in New Orleans. It was discontinued on the 21st October, while the disease was still in New Orleans, and goods in that city of necessity more imbued with the poison or contagion, and while the disease was known to be in *Vicksburg* in an increasing degree, and springing up in various parts of the South.

This statement shows the supreme absurdity of that quarantine.

Cases of yellow fever occurred in my practice between the 12th and 26th August, and between the 9th and 15th October. Dr. Lyle had cases under his care, and Dr. C. L. Smith had one case. These, in all, about fifteen, were of *bad* origin. Dr. Holcombe may have contracted the disease in Natchez, or at Lambdins, six miles below the city, in Louisiana, where it was said that yellow fever prevailed, but denied by some, for the very insufficient reason of its mildness.

That his case was yellow fever, was the opinion of Drs. Lyle, Davis, Fortu, and the writer. It was called dengué by one physician, and all were denied by Dr. Blackburn to be yellow fever.

Besides these cases, one was introduced which calls for all the attention of the people. It is the case of Mr. Poque, a Kentucky trader, who contracted the disease, most probably elsewhere, and was treated by Dr. Blackburn in this city. That Poque had yellow fever, is susceptible of full proof.

If Pearsall's case gave an epidemic in 1853, why did not Poque's in 1854?

I have a few words to say respecting errors promulgated with great zeal about the disease and quarantine during years preceding these three. Judge Dubuisson, July, 1845, teaches the Mayor and Selectmen, and the pupils of the Institute, that "the quarantine has been imposed seven times, to wit: in 1841, 1842, 1843, 1847, 1848, 1853, and 1854, with *complete success*, except in 1853."

Drs. Cartwright, McPhœters, Lyle, Jones, Cochran, and Thistle, authorized me to publish in the New Orleans Medical and Surgical Journal for 1848, that several cases of yellow fever occurred here during 1847.

There were at least fifty or sixty cases, and among them some of the most intense I have ever seen.

Perhaps those physicians knew more about yellow fever than Judge Dubuisson, though, perhaps the judge does not think so.

Respecting 1843, Judge Dubuisson and other medical men, and Drs. Blackburn and Davis, denied that the epidemic of that year was yellow fever. They called it Dengué. Yet, Judge D., admitted to me in 1854, that he had an attack of yellow fever that year in the city. Dr. Blackburn, also had an attack of yellow fever that year. He was attending Mr. Van Horen, who had black vomit within a few hours after. Dr. Jones and I became his physicians, which took place because Dr. Blackburn was attacked at this time. It was this *attack of yellow fever*, that gave Dr.

Blackburn his protection against the contagion of Pearsall's case, and against the "morbid miasm" from the "three" who died, and the "four" who "recovered at the quarantine station" in 1854, and against the "morbid miasms" of the hundreds he has attended since 1848, and against the poison of yellow fever, generated in the city of Natchez several times since. Few people now deny that the so-called Dengué of 1848 was yellow fever. There were about forty deaths, with hemorrhages, etc.

Within ten years we have had three epidemics, and during two years only, we have not had some cases according to my observation.

Dr. Lyle has been here twenty years, and says that during that time, he has seldom failed to see some cases every year.

Cases of yellow fever occurred in 1854, as in 1848, and in 1853, and perhaps in other years before quarantine was established. Therefore, it is neither to the discredit or credit of quarantine, that epidemics have or have not happened during those years. But this was not the case in 1855. Fortunately, the quarantine was established (and was rigidly enforced throughout,) several weeks before any case of this disease took place—nearly fifty days up to the time when the Board of Health, as advised by Dr. B., admitted that yellow fever existed in the city.

The explanation of all this, and the reason why Poque did not cause an epidemic in 1854 is, that an all-wise Providence did not intend us to have an epidemic that year, but did intend it in 1853 as in 1855. An all-wise Providence no more regards quarantines against the indigenous, domestic, local, in a word, against the endemic diseases of a country, than other vain efforts of vain and self sufficient man. The Board of Health deserves much credit for the graceful and prompt manner in which they admit that in the contest waged with Providence, they have been conquered; and more for the admission, so forcibly implied, that in 1854 there could have been no contest with Providence—in other words, that Providence did not intend an epidemic that year—only fifteen or sixteen cases of *local* origin, and one or two imported—otherwise, the Board of Health would have come out of the contest, then as now, second best, a conclusion which the Board will admit no doubt.

Having given the preceding history of yellow fever, and quarantine at Natchez for some time past, I invite the particular attention of the profession, and of all others as well, to the following correspondence.

It will be found rich in material for profound reflection, showing among other things, useful to be known, how incompetent "some of the citizens

of Natchez and its neighborhood" were to judge the question involved — perhaps, only because they attempted too hastily to settle it, *before they knew all the facts*, and did not know that ten years were necessary — one was not sufficient to test it.

It is worthy of note, that at the time of publication there were cases of yellow fever of *local* origin in four distant parts of the city. Reversing the order of theatricals, the tragedy began even before the farce was finished.

[From the *Courier* of August 22d, 1855.]

NATCHEZ, August 17th, 1855.

"*My Dear Sir* : In behalf of some of the citizens of Natchez and its neighborhood, and many of your warm personal friends, I tender for your acceptance a pair of Silver Pitchers, as a slight testimonial of the very efficient manner in which you carried out the quarantine regulations of 1854 — thereby saving our community, as we humbly believe, from that most dreaded of all scourges, epidemic yellow fever.

Trusting that your efforts may meet with a like success the present season,

I remain, sincerely, your friend,

GAB. B. SHIELDS."

TO DR. L. P. BLACKBURN.

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NATCHEZ, August 20th, 1855.

"TO MAJOR G. B. SHIELDS : *Dear Sir* : The magnificent and tasteful token of approval by my fellow citizens, of my professional and official course in relation to the establishment and management of the quarantine of 1854 in Natchez, have been received by me with the most grateful emotions of the heart. To receive the approbation of those so distinguished and influential in a community, where I, but a few years since, a stranger, cast my lot for life, is a reward rich enough not only to satisfy ambition, but also to stimulate to increased and unceasing exertions.

I cannot, Sir, claim to be the originator of the system of quarantine prevention against yellow fever in the South or the United States. The records of the State Legislation of New York, Pennsylvania and Mississippi, show that it is a doctrine much older than the date of my entrance upon the duties of professional life ; but I should have been a poor investigator of medical facts, and blind to the incontrovertible teachings of experience, if I had not become the ardent supporter of quarantine measures, sustained by my own observation during a practice of nearly ten years in Natchez.

Permit me again, Sir, to thank you and the gentlemen you represent, for the delicate and acceptable manner in which your generous approbation, of my humble labors in the quarantine cause, has been exhibited.

The gift of plate, so flatteringly presented, is but another effort in the quarantine cause, the tendency of which cannot fail to strengthen and confirm the salutary public sentiment now prevalent on this important subject, and to lend the influence of high social position and honorable names to the support of a great sanitary movement.



Wishing you, sir, and my kind friends, the best of blessings, permit me to subscribe myself,

Your obliged and grateful, humble servant,  
L. P. BLACKBURN."

"The influence of high social position and honorable names" never determined the truth, and can only succeed for a very limited time in clothing error in the garb of truth. Providence exerts a controlling sway over the efforts of all alike. The pitchers are fit only to be hung upon the willows, as a token of contrite and pious submission to Providence.

Yellow fever is going through one of its grand cycles of increase in violence, and of extent of territory invaded. It is to be feared that we have not seen the end of it yet. The disease has already appeared in Baltimore in 1853 and 1854, (there also in 1849); in Philadelphia in 1853; in St. Louis in 1854; in Norfolk, Portsmouth, Gosport and Memphis in 1855. It will soon be shown that it was of *land* origin in Gosport, as in all the other places named.

Another cycle began in 1791-3, prevailing more extensively than usual in the West Indies and Spain, and for many years, through the Northern Atlantic States. Its local origin was well shown in six different places during the embargo, and over and over again when quarantines were established.

Another cycle again may be stated in 1817-22, during which time, the disease appeared in a great many places, as Philadelphia, New York, and at Natchez for the first time, epidemically at least, and in numerous other places in the United States.

This periodical increase of the disease in extent of country it invades, is the explanation of its *communicability* by persons, goods, carpet bags, etc., so apparent to many, and yet so unreal.

The poison of yellow fever may be put up or generated in the *holds* of ships, and thus it may be, and often has been transported from port to port, or comes from the open ocean into healthy ports, giving the disease to them who go on board, or *near* the ship after opening the hatches. Infected ships have long since been known to be "floating places of infection." Let them be quarantined, and avoided by the unprotected. Let river steamers go with wind sails and open hatches, and then fear not to enter the hold, even if they started full of an infected atmosphere. It cannot remain against a ten knot breeze.

Thus far, quarantine and no farther—for belief in all else is unfounded—mere *post hoc* conclusions, and leads to panics, inhumanity, and back to ages of barbarism, and sadly increases the mortality from the disease.

## *Adversaria Medica.*

### No. IV.

*By I. L. CRAWFORD, M. D., Visiting Physician, Charity Hospital, New Orleans; Honorary Corresponding Member of the Society of Arts, of England, etc., etc.*

### AZOTURIA.

There is a form of disease, which though not generally noticed by practitioners, or alluded to by writers, I consider as one of great importance. I mean that form called by Willis, azoturia, and characterized by the presence of an enormous amount of urea in the urine. It not unfrequently happens, that a patient will call upon a physician, complaining of all sorts of anomalous sensations—pains in the limbs and back, general languor, and disinclination to labor, either physical or mental, melancholia, nervousness, and all the symptoms which characterize the most aggravated forms of dyspepsia, for which in nine cases out of ten it will be set down.

On a careful examination, we shall usually find every organ of the body healthy, but on testing the urine, it will not unfrequently be of a very high specific gravity of an unusually dark color, and if we add an equal bulk of strong nitric acid, we shall have an instantaneous crystallization—an unequivocal evidence of the presence of an excess of urea.

A very prominent symptom of the disease is a want of virile power, with all the concomitant anxieties which accompany impotence—and when we consider that urea is the result of the destructive assimilation of the tissues of the body, we shall not be surprised at the great emaciation which so often ensues—in fact, the wasting frequently goes on to such an extent as to lead to a strong presumption of phthisis, although diabetes is the disease most frequently mistaken for it. It has long been a practice with me, in every instance in which the specific gravity of the urine exceeds 1.020 to test for urea, and I have almost invariably found it present in excess. Where the specific gravity reaches 1.030 the mere addition of strong nitric acid will often be sufficient to produce a copious and rapid precipitate of nitrate of urea; where the specific gravity is lower than this, it is usually necessary to evaporate the urine to half its bulk, and then, when cold to add the nitric acid.

I have not unfrequently seen urine, ranging as high as sp. g., 1.040, and 1.035 is very far from uncommon, and I am convinced that many cases, which in colder climates run into diabetes—in this warmer region, put on the milder form of azoturia. We shall not be surprised at the converti-

bility into, or rather connection of one disease with the other, if we reflect upon the strong relations which exist between grape sugar, lactic acid, gelatine and urea. I have already alluded to the source of urea, viz: its being the result of the destructive assimilation of our tissues, and according to Prout, being the especial result of the waste of the gelatinous. Later, and perhaps more correct experiments, point to uric acid as the primary product of the waste of all nitrogenous tissues, and to urea as the secondary product arising from the action of oxygen on the uric acid. On this subject, Bischoff has the following observations :

“ Urea must be regarded as the product of the metamorphosis of the tissues, and of the decomposition of the nitrogenous principles of the organism. It is never formed directly in the vascular system at the expense of the albumen of the blood. Gelatine alone when it enters accidentally into the torrent of the circulation, which is rare, can give rise to the formation of urea in the blood itself. Although urea results from the transformation of materials already elaborated, and forming the organs, yet, the aliments exert on the secretion of this principle, a more powerful influence than is supposed. The secretion of urea, continues indeed, during abstinence, but the quantity diminishes considerably — thus a dog, which had devoured 4,000 grammes of beef, without fat or bone, secreted in twenty-four hours 190 grammes of urea — while, on a diet consisting of 500 grammes of potatoes and 950 of fat, he eliminated only from six to eight grammes of urea. In addition to this, the quantity of urea eliminated depends to a certain extent upon the quantity of urine passed. Salt also increases the quantity of urea excreted — a dog, who was fed upon a pound of meat, and secreted 22.50 grammes of urea, excreted 28.34 when 12 grains of salt were added to his food.”

From these remarks of Bischoff, we may discover some useful rules for treatment. Much as the disease perhaps accords with diabetes in its physiological view, its treatment, at least so far as regards diet, must be the direct opposite. In diabetes, a diet rich in starch, and poor in nitrogen, increases the amount of sugar in the urine — in oxaluria, the same diet diminishes the amount of urea excreted, and consequently the waste of tissue. We have in fact in the treatment of this disease, to diminish the tendency to increased destructive assimilation, while at the same time we give tone to the whole system. Under this disease, a patient is perhaps in a condition similar to what he would be were he placed in an atmosphere of oxygen gas, in fact, he lives too fast, and in commercial phrase, while at the same time one increases his available income, one must diminish his expen-

diture. I have not found the same benefit from the use of opiates, as experienced by Drs. Prout and Rees in the short notices they have given of this intractable, and in my experience, comparatively frequent disease. My usual treatment has been, firstly, in order to counteract the slight congestion, or rather torpidity of the liver which usually accompanies this affection, to commence with mild purgative alteratives, preferring the hydrarg. c. creta, with a few grains of rhubarb, night and morning, and then to administer ferruginous tonics in large doses, preferring the iodide of iron on account of its mixed alterative and tonic action, and in some cases, administering at the same time the sulphate of zinc, in from five to ten grain doses three times daily. The zinc has the advantage, that while exerting a powerfully tonic influence, it seems to possess a slightly sedative power, especially over the spinal and ganglionic system of nerves. We see this action powerfully exemplified in the treatment of chorea, which yields more surely and readily to the influence of zinc, than to any remedy with which I am acquainted.

In addition to this, as we find from Bischoff's observations that the increased excretion of urine necessitates an increased excretion of urea, we must limit the amount of liquid imbibed, and if, as sometimes occurs, the patient be tormented by thirst, a lemonade made with phosphoric acid, will relieve this very rapidly, and with a very little expenditure of fluid. With regard to diet, it should be nutritious, but unstimulating. Plainly broiled meat, with a liberal allowance of vegetables and stale bread, or what is better, bread made with the unbolted wheat meal, should be the staple of the meal, and chocolate, tea, coffee, and such like highly nitrogenised substances rigidly excluded. The warm or cold bath, and the use of the flesh brush, should also be recommended, and a moderate but not excessive amount of exercise in the open air. I believe, that especially in this country, an injunction to sleep for half an hour after dinner, would in many instances be attended with the happiest results. Contrary to the popular opinion, I feel sure, that a state of perfect quiescence is the means best calculated to ensure perfect digestion and assimilation of food. In illustration of the foregoing views, I append the following case.

On the 6th December, 1852, Mr. R., called upon me, he stated that for the last twelve months, he had been entirely out of health—he suffered from an unaccountable debility which was constantly increasing, and hardly allowed him to attend to his business—he was becoming daily, more and more emaciated, his memory lately, had become very bad; he was completely impotent, was subject to the most distressing hypochondriacis, and



was constantly haunted with a desire to commit suicide — his appetite was good. What made his case the worse was, that he was engaged to be married, and feared his loss of virile power, which to him appeared the most fearful symptom, would necessitate the withdrawal of his engagement. On examination, the lungs and heart were perfectly sound, the pulse somewhat quick and irritable, the tongue rather morbidly red at the edges, and too clean. He did not think he passed more urine than natural — the bowels were somewhat costive. His previous life had been tolerably regular. I requested him to bring me a bottle of his urine, viz: a mixture of the night, and early morning urine, so as to have an average sample of that passed in the twenty-four hours. On examining it, I found its specific gravity 1.030, very clear, and of a dark amber color, highly acid, and containing an enormous excess of urea. An ounce gave an almost immediate precipitate with nitric acid, of nitrate of urea, and when the same quantity was evaporated to half its bulk, and an equal volume of nitric acid added, *it became absolutely solid*. It also gave a copious precipitate with tincture of galls, showing the presence of a large quantity of extractive matter, viz: creatine, creatinine, etc. I ordered

Resinæ scammonii	. . .	3 ss.
Hydrarg. massæ,	, . .	gr. xii.
Camphoræ,	. . .	gr. xxiv.

To make 15 pills, 2 to be taken night and morning.

And after a few days,

Syrupi ferri ioididi,	. . .	3i. ss.
Tinct. aurantiæ,	. . .	3i.
Aquæ menth pip,	. . .	3iii. ss.

A table spoonful to be taken three times daily.

December 14th — There was very little improvement, except that his bowels acted regularly, and he slept somewhat better. I ordered

Acidi. nitro-muriatic,	. . .	3i. ss.
Tinct. cinchonæ comp.,	. . .	2½ oz.
Infus. gentian comp.,	. . .	3 xvi.

A wine glass full three times daily, and

Ferri. per hydrogen reduct,	. . .	gr. xxiv.
Hydrarg. massa,	. . .	gr. xii.
Ext. rhæi,	. . .	gr. xxiv.
Ext. conii,	. . .	3i.

To make 24 pills — 2 three times daily.

December 21st — Urine sp. g., 1.027, light amber color. Amount of urea less than on the 6th. He continued with this or similar treatment until the 4th of January, there being occasional alternations in the density

of the urine—but its usual sp. g., being now about 1.027, and the amount of urea moderate. Ordered.

Zinci sulphatis,	- . .	gr. ii
Ext. nucis vomicæ,	. . .	gr. i.
Ext. conii,	. . .	gr. iii.

To make a pill, to be taken three times daily.

And the syrup of the iodide of iron in drachm doses, three times daily, after meals—occasional doses of aloes and blue mass. The sulphate of zinc was increased by a grain in each pill every other day.

January 8th, 1853 — Urine sp. g., 1.024 acid, amount of urea much diminished. Ordered—

Infus. gentian comp.,	. . .	12 oz.
Infus. senna comp.,	. . .	3 oz.
Tinct. nucis vomicæ,	. . .	} aa 3ii.
Tinct. opii,	. . .	
Tinct. cardamom comp.,	. . .	1 oz.

A wine glass full three times daily, and

Zinci sulphatis,	. . .	gr. v.
Ferri sulphatis,	. . .	gr. iii.
Ext. conii,	. . .	gr. v.

Ft. pil. ii, to be taken three times daily.

January 16th — Urine sp. g., 1.020. No urea to be detected in addition of nitric acid. Color natural—is now taking ten grains of sulphate of zinc three times daily, with the sulphate of iron and the mixture of the 8th.

His health has considerably improved—he has gained much in flesh—his chief fear now is his supposed impotence, he says, he finds it impossible “to have an erection,” and is apprehensive he will not be able to fulfil his engagement, as he was not to be married for three months. I recommended him to cohabit with some female every night for a week, but not to attempt intercourse until the end of that period, and to continue his medicines.

January 23d — Called to say he was convinced he was perfectly able to fulfil “the duties of a husband and father,” that he had found it rather difficult to comply with the injunction after the first three nights, but had nevertheless obeyed. I considered him convalescent, and a few weeks after, he departed for the North in order to get married. I saw him on his return, and he was then perfectly healthy, and in the fullness of time, evidence of his restored vigor became distinctly manifest.

## Prolapsus Uteri.

(COMMUNICATED.)

*Messrs. Editors:* The following extract from a published lecture by Dr. Barker, Professor of Midwifery and Diseases of Women and Children, I think is worthy of the consideration of the profession. If you agree with me in opinion, please insert it in your Journal:

“In the beginning of the lecture, following Vidal, I divided the causes of prolapsus into those inherent to the uterus itself, (that is, all those causes which augment the volume or weight of the uterus,) those which refer to the state of the vagina and pelvis, and those which refer to the attachments of the uterus.

“Now, the indications for cure must be based on the causes which produce the displacement. Prolapsus, resulting solely from the condition of the uterus itself, usually, I think, subsides spontaneously when the uterine trouble is removed. I have often found the uterus very low in the pelvic cavity when there is inflammatory disease of the cervix, but rising higher and higher during the treatment. I may add that, so far as my experience goes, this is a much more frequent cause than both the others; but it is to prolapsus depending upon the other causes to which I wish now more especially to call your attention. That there are causes entirely independent of the weight of the uterus, I am perfectly certain; as I have in several instances seen *complete prolapsus* where the uterus was evidently atrophied. Admitting then, both the causes before mentioned, the indications for cure will be, 1st, to retain the uterus in its normal condition; 2d, to diminish the preternatural capacity of the vagina; and 3d, to restore tone to the ligaments. The first is gained by mechanical means, the second by astringents, and the third by fulfilling the first two, and increasing the general vital powers. Very slight support suffices to fulfil the first. I have rarely found any evil resulting from the attempt to accomplish the second, such as injury to the general health from arrest of the accustomed discharges, or inflammation or irritation of the mucous membrane of the vagina. The method which I adopt is the following: I cut out a double thickness of patent lint of a triangular form, so that when rolled up it will form a cone, of a size adapted as nearly as I can judge to the capacity of the vagina. Half an inch from the apex is firmly tied a piece of narrow bobbin, for the purpose of facilitating withdrawal. This is soaked in a saturated solution of tannin. The patient being placed upon her back, the

uterus is replaced, care being taken to adjust it so that its axis corresponds with the axis of the superior strait, and the lint introduced with the apex first; but after it is in the vagina it is turned, so that the base will come under the os tincæ. This is withdrawn, and a new one introduced, twice in the twenty-four hours. In some cases there is soreness and tenderness of the vagina, when I add to each ounce of the solution of tannin ℥ii of laudanum. I have used morphine, but the laudanum seems to be more efficient in removing the soreness. The size of the lint pessary is gradually diminished until the base is not more than half an inch in diameter, when the cure may be considered as accomplished. This should never be left for the patient to do herself. It requires the personal attendance of the physician. The patient will not do it properly or efficiently. You will surely be disappointed if you trust her. If she have means, she will not demur at paying for all the trouble you are at in effecting a cure; if she be poor, you will be amply repaid in seeing her able to perform her duties in life with comfort and ease.

"This is a very different mechanical support from the sponge, which expands in the vagina, or any unyielding pessary, or even the sachet 'filled with finely grained, not pulverized, Aleppo galls,' of which Professor Meigs speaks. This *contracts* in the vagina. It, so to speak, *packs* in the vagina, so that when you withdraw it, you will find it much smaller than when you introduced it. Indeed, I am sure you will be surprised to find how rapidly you are obliged to diminish the size of the lint. But local treatment is not all that is necessary. I need hardly say that, previous to commencing this treatment, the bowels should be thoroughly evacuated, and that during the whole treatment they should be kept *well opened*. Every man of tact and discrimination will adapt his general treatment to the peculiarities of his patient. Many of this class require tonics. To some I have given three times a day two grains of quinine, in a wine-glass full of the solution of the citrate of magnesia. To others I have given the tart. or the citrate of iron in the same solution. Some I have given the iodide of iron, and recently I have been greatly pleased with the effects of the maganese as a tonic. All do not require tonics. But above all things keep the bowels open, and even after you cease attendance, threaten your patient with all the terrors of a relapse, if she do not keep her bowels open.

"Formerly I used to direct my patients to keep the recumbent posture during the first week of treatment; but on finding that my poor patients, who were obliged to keep about, got along better than those in better cir-



cumstances, I have now adopted a different course, and send them out into the open air as much as possible, from the beginning.

“This mode of treatment is applicable to each of the different degrees of prolapsus. I have before remarked that the symptoms, where there is but slight depression, are quite as severe in some, as those attending complete prolapsus are in others. I have several times been led to suspect, from the severity of the symptoms complained of, that inflammatory disease of the cervix existed; but a careful examination with the speculum revealed no disease. The pain in the back, nausea, fever, vaginal irritation, and constipation, were the result of the depression. Lisfranc declares that all cases of incipient prolapsus are caused by congestion. He directs that the congestion of the uterus should be first treated, and if after that the displacement of the womb continues, the pessary may be applied if the patient can bear it. Now, the lint and tannin pessary applied in the manner in which I have directed, relieves this condition of things at once. To borrow an illustration from Dr. Meigs, it acts like a suspensory in the treatment of orchitis.

“In complete prolapsus, you will be able to use this means of treatment when no other form of pessary can be retained or worn. Sometimes it will be necessary for the patient to wear for a time a perineal bandage, but this is not often the case. Please to try this method, and see if you can not in all cases effect a radical cure. I believe you can, in all cases excepting those where the sacrum is very straight, and there has been great loss of the substance of the perineum.

“I could give you the history of many cases of complete prolapsus, where a perfect and radical cure has been effected by this plan. A widow, aged thirty-two, cook for a large and fashionable boarding-house in University Place, had the uterus entirely protruded from the vagina. She was obliged to wear a napkin constantly, to keep the uterus within the vulva. It protruded at once on removing the napkin. She was cured in two months, by the plan I have described. Soon after, she married a waiter in the house. The third of July she went on as teamboat excursion, and danced a good deal. I was called to see her on the fourth, on account of a severe flooding; and she miscarried with a five month foetus. She resumed her duties as cook within a week, but there was no return of the prolapsus.

“An old lady, sixty-eight years of age, residing in Columbia street, had suffered with complete prolapsus for more than twenty years. Various

kinds of pessaries had been at different times adjusted by men of eminence in this city, but for five years she had been unable to wear any. I found her in bed — where she passed the greater part of the time — the uterus small, but the whole tumor external to the vulva, formed by the uterus, vagina, part of the bladder, and part of the rectum, was as large as the egg of a goose. If the tumor was pushed back while lying on her back, it immediately returned. The mucous membrane of the vagina was superficially ulcerated in two places, in one to the size of a twenty-five cent piece, and the other considerably smaller. The cure was effected in three months. The last time I saw her, she said that there was no tendency to falling; and she had left off for some weeks the perineal bandage which I had made for her.

I am often asked, by friends with whom I have conversed in regard to this plan of treatment, if I have never met with evil consequences from the suppression of the profuse discharge which usually attends the prolapsus? I have, two or three times, but not within the last five years. Formerly I was less careful than now, to use laxatives *freely* during the whole course of treatment."

The remarks of the author upon the importance of avoiding constipation should be particularly borne in mind. From many causes the female more frequently suffers from constipation than the male, and the consequences are the more serious as she has an additional and important organ to be influenced by it. Constipation and its consequences to the rectum itself not only produces many of the symptoms that are, in this age of uterine hobbies, referred to the uterus, but is a fruitful cause of prolapsus. Tenesmus and persistent bearing down, whether it is caused by constipation or the state of the uterus itself is the real or most common cause of prolapsus.

W. STONE.

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## Yellow Fever in the Country:

*An Account of the Disease as it prevailed at Judge BAKER's Plantation, Parish of St. Mary, Louisiana, in September and October, 1854.*

By C. R. FASSITT, M. D.

The plantation of Judge Baker is situated nine miles below Franklin, on the bayou Têche. His residence is on the west side of the bayou, which

here runs north and south. The negro quarters are below the residence, the nearest being a hundred yards, and the farthest about three hundred and fifty yards distant. The bayou is a hundred yards wide, and its shores connected by a bridge. On the east side, there is also a row of cabins, situated some sixty or seventy yards from the bayou, extending some two hundred yards. Opposite the upper part of this row of cabins, is a railroad for hauling out vessels.

There were about one hundred and ninety negroes on this plantation, one hundred and thirty of whom had yellow fever. Six died, of whom, two had black vomit, one a mulatto, and the other a black negro boy, aged about 10 years. One other was said to have vomited black matter, but I did not see the matter ejected.

This plantation is not usually more sickly than others in the neighborhood. The negro houses have plank floors, and are as comfortable as such houses usually are found.

There had been no disease prevailing to any extent before the appearance of the fever in September. The houses are so situated as to form quite a town, and the streets during the whole of the month of September, were in a most wretched condition, being a foot deep in mud.

The origin of this fever is the only matter of special interest connected with its history.

The barque *Tivoli*, sailed from New Orleans, towards the last of August, and reached Last Island, where, on account of sickness on board, she came to anchor, and there remained until the 5th of September. Two men died of fever on board, and the captain was brought to Centerville, having had fever also. He was very yellow and much debilitated, and one of the physicians who saw him, told me that he considered his disease yellow fever.

On the 5th of September, the *Tivoli* was towed in from Last Island by Judge Baker's steamboat *Picayune*, and left at his plantation. Here, at the railroad, on the east side of the *Têche*, she was overhauled and cleansed while within a hundred yards of half a dozen negro cabins.

The hands of the *Picayune* (all of them negroes and mulattoes,) were the first who were attacked with fever. They had been on board of the barque, and had cleansed and smoked her hold, and were taken sick within ten days after.

I did not see them until the 26th September. I was then told that all of the hands of the *Picayune* had been sick, but were recovering slowly; for although they had no fever for several days, they were very much debilitated.

One mulatto, who had had but little fever, complained very much of prostration, and I found his pulse at forty-five per minute. Another man, also a steamboat hand, had slight bleeding from the gums. There were many new cases at this time, in all, some twenty negroes soon "laid up with the fever." The number of cases steadily increased until the 20th of October, when about fifty negroes were in the cabins. The first death occurred on the 4th October, and the last on the 17th November.

On the 17th of October, two cases occurred in white subjects. M. Anthony Baker and his cousin, were both attacked on that day. The former had black vomit in two and a half days from the onset of the disease, and died on the fourth day.

The latter died on the ninth day, having had hemorrhage from the nose and gums, and black vomit. There were seven cases of fever among white subjects, and four deaths.

The negroes for the most part were not seriously affected by the disease; many had the fever only twenty-four hours, but in every instance, greater prostration was observed than is usual in other fevers.

The negroes in the cabins near where the Tivoli lay, were first attacked by the disease, and I did not see a single case in which the subject had not been on the east side of the bayou.

I do not believe that yellow fever is *contagious*; that is, that one person having the disease can under ordinary circumstances communicate it to another. But, here the cause of the fever, whatever it may have been, was evidently in the vessel, and was communicated not only to those who cleansed the hole, but also to all persons in the vicinity. The cause remained after the vessel had left, and evidently increased in intensity.

Another reason why the disease became more fatal among negroes was, the fact that owing to the great number sick at one time, it was impossible to have them all well attended.

C. R. FASSITT, M. D.

The above interesting communication was made to Dr. E. D. Fenner, who has kindly handed it to us for publication. Dr. F., has given us the following additional facts in relation to the barque Tivoli, which he obtained from a reliable source.

The Tivoli took a cargo from New York to Chagres. After discharging she sailed for Berwick's Bay, on the coast of Louisiana, for the purpose of getting a cargo of molasses to take back to New York. After leaving Chagres, sickness broke out on board, supposed to be what is called "*Cha-*



*gres fever*," a severe bilious remittent, and the distress was so great, that the vessel had to be taken to New Orleans. She arrived here in the midst of the epidemic yellow fever. A new crew was obtained, and she again put out for the bayous emptying into Berwick's Bay. It is presumed this new crew consisted of unacclimated men, and the vessel had hardly cleared the mouth of the river before yellow fever broke out on board, and prevailed to such an extent, that she had to be left to the mercy of the waves. She was cast on Last Island, from which she was taken by Judge Baker's hands. What occurred afterwards is graphically detailed by Dr. Fassitt.

EDS.

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## Yellow Fever in the Country:

*An Account of the Disease as it prevailed on the Coast below New Orleans, in 1854 and '55.*

By D. R. FOX, M. D.

DR. FENNER: *Dear Sir*: In compliance with my promise, I will endeavor to give you an account of the yellow fever as it has occurred in this parish; but before doing so, I think it proper to give a general description of the parish, its inhabitants, and its most prevalent diseases. This parish comprises all that narrow strip of land bordering the Mississippi river on both sides, extending from within twenty miles of the city of New Orleans, to the mouths of said river, being about eighty miles in length, and averaging six miles in width. The tillable land extends only thirty acres in its widest portion, and about ten acres in its narrowest part back from the river; behind this, the land is too low for cultivation, is partially covered with water during the winter season, and is affected by the tides of the Gulf. During the summer it is comparatively dry, and is covered with a species of course grass. This prairie land, as it is called, extends only a mile or so back of the arable land; beyond this, the land becomes low and marshy, covered by coarse reeds, and is intersected by a perfect labyrinth of bayous or natural canals which communicate with the gulf. The distance from the river to the gulf, I do not think, is more than ten miles in its widest part, and one or two miles in its narrowest part.

This parish contains forty-three sugar plantations, producing from twelve thousand to eighteen thousand hogsheads of sugar annually. In its lower portion there are many small rice farms, and this section produces more rice than any parish in the State. Oranges are grown also in considerable quantities, and contribute largely towards the supply of the city of New Orleans.

*Inhabitants.* — This is probably the oldest settled portion of the State, and contains a greater mixture of the *races* than any other region in the United States. The largest proportion of these are descendants of the French. The balance consists of Anglo-Saxon, Spanish, Italians and *Africans*, and a *mixture* of these in all grades and proportions. There is a large *free* colored population in this parish. Scattered along at intervals between the plantations, are many small neighborhoods or settlements as they are called, consisting chiefly of the native creole population. Of these settlements, there are six, which deserve more the name of small towns or villages.

1st. The Jesuits' Bend settlement; 2d, Pointe à la Hache; 3d, the Ronquille settlement; 4th, the Burat settlement; 5th, the Balize; 6th, the South-West Pass. The Jesuits' Bend is situated on the right bank of the river, (going down stream,) and is twenty-three miles from New Orleans. It is divided into two portions, the upper portion being separated from the lower by two sugar plantations, making an interval of two miles between the two. The upper portion of the settlement contains eight families, within the space of six acres fronting the river, numbering in all seventy or eighty souls. The lower settlement contains about ten families, numbering in all between seventy or eight persons.

The next village is Point à la Hache; it is forty miles from the city; it is the county seat; has a court house, jail and post office; and contains within the space of a mile, probably two hundred persons. This village is on the left bank of the river.

The Ronquille settlement is on the right bank, and contains nearly as many inhabitants as Pointe à la Hache. The Burat settlement is seventy miles from the city, on the right bank of the river, is in the vicinity of the present quarantine station and has about fifty families. Finally, at the mouths of the river, are the Balize and South-West Pass settlements, consisting of pilots and their families, numbering in all from twenty to thirty inhabitants.

*Disease.* — By far the largest portion of the diseases of this parish are, miasmatic, chiefly intermittent and remittent fevers, and so far as my ex-

perience goes, I have found them to yield readily to proper treatment, and they are not so often associated with local inflammations or congestions as I have observed in some other parts of the State. The next most frequent diseases are, dysentery and diarrhea, particularly among the negro population. Many cases of Asiatic cholera occur annually, *it prevails* also more among the negroes than whites, and is more fatal to the negroes. More cases occur in the spring (May and June,) than at any other time. This disease is sometimes endemic on a plantation, while the adjoining ones would be singularly exempt from the disease.

Pneumonia occurs during the winter months, but is generally mild and easily treated. Worms are very prevalent, particularly among the negroes, old and young. Tetanus, both idiopathic and traumatic, is of frequent occurrence, mostly among the negroes. (The charbon or pustule maligne) has occurred here frequently; it is a common disease here among the horses, mules and horned cattle; a planter sometimes loses all his stock. In regard to the yellow fever, I have learned some interesting facts. Situated as this parish is, so near the city, and there being a constant intercourse between the city and the various plantations and villages, by the means of numerous sailboats which stop along the coast, carrying produce of various sorts to New Orleans, and bringing merchandize to the numerous settlements, and also by a steam packet, which regularly stops once a week at every plantation and village between New Orleans and the Balize, one would suppose that yellow fever would be a common disease among its inhabitants; yet, upon diligent inquiry among the oldest and most intelligent persons, I am informed that the disease has never shown a disposition to spread until the year 1854. I have heard of many cases occurring in persons who having visited the city, have returned home with the seeds of the disease in their systems, and have died throwing up black vomit. They were surrounded by their relations and friends during their illness, and yet no one took the disease from them. And even in 1853, when this disease was spreading terror and dessolation in the upper parishes, it did not prevail here, although there were several cases in persons who had contracted the disease in the city. I may mention that this disease has never prevailed among the negroes; at least, no deaths have occurred among them from this disease in this parish.

Another interesting fact was related to me a few days ago by an intelligent planter, (a creole, who has resided here all his life,) he says, that within ten or twelve years, many of the creoles here have sold their small farms

to the sugar planters, and have gone since to reside in New Orleans. He can count upwards of twenty persons of his acquaintance, who, although many of them have now resided in the city several years, yet, upon diligent inquiry, he can learn of *one case* only of yellow fever among them.

In 1854, our parish seems to have lost its immunity from this pestilence. I was called on the 12th of September, 1854, to see an Italian, named Vincent, resident with a creole family. This family is in the upper Jesuits' Bend settlement, and consists of six persons, viz: the mother, a son, eighteen years old, two younger children, (one girl, ten years, and a boy eight years,) also a married daughter, aged seventeen years, and her husband, (an Italian.)

*History* — I learned this man had come from the city the day previous, and that he was unacclimated; had never spent a summer in New Orleans; had been in the United States four years. He had all the symptoms of yellow fever well marked, and was sick eight days in all; he recovered.

*Case 2d.* On the 18th of September, the little girl, aged ten years, was taken ill with the fever. I saw her twelve hours after, and she had unmistakable symptoms of yellow fever. She threw up black vomit, and died forty-eight hours after she was first taken ill.

*Case 3d.* The married daughter, aged seventeen, (enceinte six months,) was taken sick on the 24th with fever. The symptoms though well marked, were mild. The fever lasted fourteen hours, and then subsided. I gave her quinine freely. The fever did not return.

*Case 4th.* The little boy, aged eight years, was taken on the 27th, having all the characteristic symptoms of yellow fever; was ill six days; was convalescent on the 4th. On the second day after the fever left, his pulse was only fifty a minute. I gave him stimulants, and broth as nourishment. He improved, and became rapidly convalescent.

*Case 5th.* The mother was taken on the 29th. Symptoms similar to the others. Was convalescent on the fifth day.

The young man, aged eighteen, left the house as soon as the Italian was taken, and did not return for two weeks; he escaped the fever. The nearest house above, was half an acre; it contained a large family, fourteen persons in all; no one had the fever.

Another family, consisting of some fourteen or fifteen persons, residing half an acre below, became alarmed as soon as the nature of the disease was known, and left their house and went into the lower settlement; no one had the fever. No cases of fever occurred out of the infected house.



As soon as the nature of the disease was known, no one visited the house. A neighboring planter kindly sent a negro woman, who was acclimated, (having lived in New Orleans,) to nurse the sick.

I wish I could give you a correct account of the fever as it occurred in the other settlements, but I have tried in vain to get at the particulars. All that I have learned is, that ten days after the disease made its appearance in Jesuits' Bend, it occurred in the Burat settlement, the fourth village from the city. It is said to have been brought there in the same manner that it was brought to the Jesuits' Bend settlement, viz: by an Italian boatman. It was very fatal, only two or three persons recovered. More children than grown persons had the disease. The physicians of the Ronquille settlement, which is twenty miles above the Burat, (the late Dr. O. Hags,) was sent for. He paid two visits at the village — contracted the disease, and died about forty-eight hours after he was taken ill. He threw up black vomit freely. He was attended by Drs. Egan and Boyer, who can inform you of the particulars of his case. He was visited and attended by his *friends* and relations, yet, no one else had the disease in the Ronquille settlement. About two weeks after the disease in the Burat, it prevailed at Pointe à la Hache, the second settlement from the city; it was thought to have been brought from the Burat settlement. The above were all the settlements in which the disease prevailed in 1854.

*September 3d, 1855.* During the last week in August, I was called to see seven cases of fever in one house, all sick at the same time, three white, and four blacks. Looking upon them as mere cases of bilious intermittent fever, I prescribed for them accordingly, ordering quinine to be freely given immediately on the subsidence of the fever. All these cases did well. Two days after, while passing the house, I was called in to see another member of the family, a young lady, of eighteen years, and found her extremely ill. She was throwing up black vomit, and passing the same from her bowels. She had been taken ill three days previous with fever, accompanied with violent pain in the head, back and calves of the legs. She was treated by her mother with the usual creole remedies in such cases, viz: hot pediluvia, cold applications to the head, frequent sponging with tepid water and vinegar. She had been given an emetic of ipecac, followed by a dose of epsom salts, as her fever had continued without remission — she had taken no quinine. I found her restless — much frightened — skin hot and dry — pulse one hundred and twenty, but feeble — tongue dry, but not furred. She complained of soreness in the fauces. Schlerotic slightly yellow —

tenderness over epigastrium — suppression of urine. I cupped over the seat of pain, and the pain continuing, applied a blister over the abdomen, dressed it with morphine cerate — gave her ice to hold in her mouth. She continued to throw up black vomit at intervals of four or five hours, and died thirty-six hours after the black vomit began. She was given strong chicken broth as nourishment, and stimulants also. During the intervals of vomiting, she was quiet and free from pain. She became extremely yellow after death.

As soon as this person died, the family removed to a vacant house, one acre distant from their dwelling. In six days after their removal, a younger sister was taken with the fever, and within the space of ten days, every member of the family had had the fever — white and black, fourteen in all. The following were the general symptoms of nearly all the cases, in some, the disease was much milder than others.

The disease began with violent pain in the head, back and limbs; the face flushed. The fever began with the pains; the eyes were more or less red; the pulse varied from one hundred and eight to one hundred and twenty; the tongue in general was moist; bowels rather torpid; the skin was hot, but easily made to perspire. In some cases, there was pain over the epigastrium, and some nausea at the beginning of the attack.

*Treatment.* — I pursued the abortive treatment. The family had become alarmed, and I saw every case early. My plan was, first, to give a mercurial purge, blue mass or calomel, followed in four hours by castor oil. I endeavored to promote free perspiration, by means of hot mustard pediluvia and warm orange leaf tea, and keeping the patient under cover. As soon as free perspiration was established, I gave thirty grains of quinine in two doses, one hour or so apart. I gave it in this way for fear the stomach would reject the thirty grains at once, which was done in one or two instances. Under this treatment, the patient was relieved in from fourteen to twenty hours. I kept them in bed until after the fifth day — on the second day, I gave the following diaphoretic mixture to keep the skin moist.

R	Aqua. Camphoræ,	.	.	.	.	1 oz.
	Spt. nit. dulc.,	.	.	.	.	} aa ℥ss.
	Spt. mindereri,	.	.	.	.	
	Paregoric,	.	.	.	.	℥ss. ℥

A table spoonful every two hours.

I gave them ice to hold in the mouth when it could be had; gave mucilaginous drinks, sometimes acidulated with oranges, (green.) On the second day, gave chicken broth, gradually increasing it on the third and

fourth day. Some of the cases were exceedingly prostrated. To these, I gave stimulants cautiously, (usually gave brandy julep.) Besides the members of this family, I had four cases in the adjoining houses, most of them had been with the sick family as nurses. Free communications was kept up between the families. No precautions taken to isolate the disease. There was a great deal of prostration considering the short duration of the disease; more so than in the ordinary fevers of the parish.

In searching for the cause of this disease, I learned that the family had a negro woman hired in New Orleans. This woman came home sick with a mild fever, about six days previous to the first cases of fever in the house. She brought her bedding and many articles of merchandize with her. The woman soon recovered, and returned to the city. The first cases of fever in the family were among the negroes, in them the disease was more mild than in the whites. Up to the time of the sickness in this family, the neighborhood had been remarkably healthy, and the *black vomit* case is the first and only white death in my practice this year.

Thus, I have given you an imperfect account of the yellow fever as it has occurred in this parish. I have endeavored to give all the facts in regard to the supposed origin of the disease. I do not pretend to *theorise* upon them. I only vouch for the accuracy of my own cases.

I did not deem it necessary to go into the minute detail of every case, with symptoms and treatment, although I have taken notes on most of them.

Hoping you will make due allowance for all imperfections,

I remain, your friend,

D. R. FOX.

## Spontaneous Luxation of the Hip Joint.

*Reported by W. H. BERTHELOT, Student, Charity Hospital.*

Michael Griffin, aged eight years, of rather a scrofulous diathesis, was brought to the Charity Hospital on the 15th of August, by his mother, from whom, the following facts were obtained.

The boy, about nine months previous, had complained of some pain in the region of the left hip joint. The pain soon subsided, but the boy was found to be lame. At first, nothing was thought of the boy's limping, as

the mother was under the impression that he must have received a blow upon the hip. The lameness continued to such an extent, that in the course of three months, the child was unable to walk without assistance.

The boy was placed under the care of several physicians, but it seems as if they could not understand the nature of his disease, for, at the time of his admission he was in a sorry plight. The left leg was shortened; the knee partly flexed; the foot inverted; he could only hobble about on one leg. A careful examination of the region of the hip, plainly showed that the head of the femur was resting upon the dorsum of the ilium.

By holding the pelvis steady and rotating the femur, the head of the bone could be brought down and thrown forward into the obturator foramen. Not only could it be placed in this position, but into any, as the head was out of the socket. You could produce any of the dislocations that the hip joint are liable to.

The case was pronounced to be one of spontaneous luxation of the hip joint.

On the following day, the head of the bone was brought into its normal position, but would not remain there. It would drop from its socket without there been any effort on the part of the patient to move his leg.

This showed plainly that the disease lay in the ligaments belonging to the joint. Thus, the tenacity belonging to them was wanting.

On the 17th, two large blisters were placed, one upon the gluteal region, the other on the internal border of the hip. They were kept open for a week. And on the 23d, the head of the bone being placed in its natural position, was retained there by a Liston splint, which was kept upon the limb for three weeks.

At the same time, the following medicines were used.

R	Ol. morrhue,	.	.	.	.	.	℥iv.
	Calcis phos.	.	.	.	.	.	℥ss. ℥

A tea spoonful to be taken three times a day.

At the expiration of three weeks, the boy had greatly improved, and the splint being removed the head of the bone remained in its socket. The boy could stand perfectly erect, and walked as well as ever.

Fearing that the joint might be endangered by too much exertion, the patient was allowed to go about the house with a pair of crutches, the cod liver oil and phosphate of lime being continued. On the 16th of October, the patient was discharged cured.



## Editorial and Miscellaneous.

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Self preservation is said to be the first law of nature, and it has its origin in natural weakness. So long as death seems afar off, and men can console themselves that it may stumble upon them at some sudden turn, as an accident, this law is not called into very active force, but so soon as the Avenger spreads his black wings over the land, and with outstretched arm commands the pestilence upon them, the lights of heaven seem all put out, and they seek to fly from beneath His shadow.

We have seen whole towns and neighborhoods panic struck at the first appearance of His shadow upon the distant horizon; their miserable souls shrinking into their trembling bodies, and ready to take flight, even before He should touch them.

But, this is not the worst result of these personal fears. Frequently, under their influence, men become savages, destitute of natural affection, and practice barbarities worthy of their (not changed, but) fully developed baser nature. In their dread of infection, they drive from their doors the sick, suspected of having the disease, and (God forgive them!) allow them to perish from neglect. In their confusion, they establish expensive and stupidly arranged quarantines, and fumigations, and purifications. And when, at last, in spite of them, the shadow glooms over them, they fly a-ghost — the husband from his wife — the child from its parent — the mother from her offspring — (*this has happened!*) the friend from the friend of his bosom, leaving them to linger, and suffer, and die, without one kindly hand to moisten their parched lips with cooling drink — without one dear familiar face to look upon — to die in solitude and loneliness.

And among thousands of the living, forced by circumstances to remain subjects of the pestilence, there prevails a moral anarchy. The seeds of evil, like hideous fungi, love the obscurity, and spring up and flourish even in the hearts of those before counted moral; and drunkenness, debauchery and crime, seem to have found their most appropriate atmosphere.

We wish that we had time and space, to show fully from authentic sources, the extended and terrible demoralization caused by epidemics in other cities. The criminal statistics of London and Paris, show at times, great accessions, clearly attributable to times of general sickness, and traceable to sickly localities. It is the effect, which every one acquainted with human nature would attribute to the cause.

Nor is it alone the people upon whom the fatal influence rests, who are affected by it. They, it is true, are often left in a destitute and starving condition, by the cutting off of their regular supplies of food and comforts; but all the channels of foreign and domestic commerce which pass through their locality, are also deserted. "Ships sailorless lie rotting on the sea." The wharves are unoccupied

by their accustomed loads. Travel and the din of busy commerce cease. Ruin stares both buyers and sellers in the face. A vast amount of fixed capital of the country, and of other countries, is rendered unproductive. The poor are thrown out of employment, and abandoned to innumerable temptations to evil. The country receives a wound, not only in its financial affairs, but in its morals also, which may be healed over, but from which, it can fully recover only after long lapse of time; and as for individuals, thousands are irretrievably ruined.

What have not our own city and State suffered from epidemics of yellow fever? Were it not for these, who can place a limit to the prosperity of New Orleans? Situated in a delightful climate, near the embouchure of the greatest system of inland navigation on the globe: the natural depot of the vastest proportion of the agricultural productions which go to supply the wants, and keep in motion the commerce of the whole world; the natural channel for the import trade of the great West and South-West, what might it not now be? what might it not become? The earth would groan under its concentrated load of people and palaces, of workshops and warehouses.

But the current of commerce, arrested here, has sought other outlets: the tide of immigration has been checked; we have lost the confidence of capitalists abroad; our property has lessened in value at home; and all of our natural advantages have proved too weak to successfully combat with yellow fever.

In estimating the evils of epidemics, we take no account of the number of deaths caused by them, since all who live must die, and since, also — looking at the matter in its broadest point of view — the gross mortality of the world is lessened rather than increased by them. Yet, when we look over this country now and see how many a happy household has been torn asunder and desolated; see the mourning garments so numerous, that we seem to live in a vast funeral assemblage, hear the voice “of weeping and lamentation — Rachel weeping for her children and will not be comforted, because they are not;” we feel like exclaiming: They had to die, oh Death; yet, couldst Thou not have spared these loved ones a few brief moments longer! Thou takest first those whom we love, and then takest us away!

And when we reflect that there is not merely a possibility, but a great probability, that this scourge may be wholly prevented by a judicious use of human means; that it has been prevented elsewhere by hygienic measures suited to those particular localities, we feel that a great amount of blame is due to the governments of the States where the disease has existed.

There can be no doubt of the power of the State Governments to act in this behalf. Indeed, to do so is imposed on them as a primary and sacred obligation towards the individual members who form the societies and have created the governments; and there is also imposed on them the further natural obligation to preserve and perfect their own prosperity as States. These many years have the fearful people been crying aloud for escape. Religion, good morals, humanity, agonized parents and friends, have all been lifting up their voices for the governments to fulfil their solemn irrevocable obligations! What more do they wish to

induce them to act promptly and wisely? Why should ye be stricken more and more? Let our sinking commerce, our distressed financial condition, make a last appeal.

If it is the obligation of the State of Louisiana, what excuse can be found for its non-execution?

We will be referred to the thousands of dollars spent by the State upon Quarantines; to the long-winded speeches made in her Legislatures on the subject of Public Health, by gentlemen who can make long-winded speeches on any subject whatever at a moment's notice. Doubtless, our Legislators showed themselves duly solicitous to do their duty, by discussing the matter from the hustings before a people as ignorant as they themselves were. Doubtless, gentlemen of the press thought themselves vastly philanthropic when they vociferated Quarantine! Quarantine! as they also thought themselves afterwards when the quarantine detained a few vessels down the river, and they cried even more vociferously for, No Quarantine! No doubt, but that we all, doctors, lawyers, preachers, politicians, editors, and people, and the government, ruled by us, have been trying to do our duty, yet, we have come far short of it. We have all started off on a mad hue-and-cry after a wily criminal, when we should have been, Foucher-like, plotting in secret for his detection and arrest. We have resorted to stump-speaking, appropriation bills, editorial squibs and bickerings, to settle a matter which requires the most careful and elaborate collections of facts, and the keenest scientific investigation.

We have said that epidemics of yellow fever have been prevented elsewhere by hygienic measures suiting those localities. The yellow fever appeared first in Philadelphia, in 1699, ninety-five years before it appeared as an epidemic in New Orleans, although it had been frequently brought here by vessels from the West Indies before 1796. From the time of its first appearance in Philadelphia, up to 1820, it was frequently epidemic in that city, as well as in New York, Baltimore, Boston, and other Atlantic cities, and it is highly probable that in Boston it was an epidemic so early as 1682. It frequently has prevailed in towns of France and Spain from early in the seventeenth century, up to within fifty years. Yet, since the year 1820, in our Northern Atlantic cities, and in the French and in certain of the Spanish towns from early in this century, it now no longer appears; or, if it appear at all, it is confined to one or two localities, a square or two in extent; or, sometimes to a vessel or to vessels in which it has being imported.

Here now are certain facts eminently suggestive for one case. The yellow fever, *and consequently the cause of yellow fever*, has ceased from where it formerly prevailed, and prevails where it was formerly unknown, although neither locality has changed latitude, longitude, nor elevation.

Again, we know that in our own city the yellow fever is in some seasons violently epidemic, in others, we have a few sporadic cases, and then, for a year or so, not a case will appear.

Even if we had space, we should adduce no more facts to sustain our position. What should the government of Louisiana do, what should it have already done,

in view of these facts alone, and of its sacred obligation to its citizens, as individuals, and to itself as a State? These facts prove what the government ought to know — every body else knows it — that yellow fever is dependent for its existence and spread upon a certain unchangeable specific cause — that this cause has been destroyed or prevented in other places. Has the government the right to argue, that although it has been destroyed or prevented elsewhere, it is impossible it should be destroyed or prevented here? Is it not, on the contrary, its duty to believe, and to argue, and to act upon the opinion, that we too can be rid of this incubus upon our people and prosperity? Its supineness has been criminal. It has been guilty of so gross negligence, that if it were subject to actions of damages, those who have lost their relations could recover exemplary damages. And although it can neither be hung nor sued for damages, yet, Nature has taken a rare revenge — it has not escaped Her inexorable action of damages.

We do not blame the State for what it has done, but for what it has not done. It was right to establish a quarantine, whatever the cost; but it was wrong to arrange it so as it could not go into operation for the season, before the yellow fever had already appeared. It has been wrong in not having a strict quarantine all the year round for these many years, whatever the cost. It is wrong in not having taken steps to discover the local cause of the spread of yellow fever among us, and in not having eradicated it. It is wrong, in fine, for having stopped at the experimental caution of quarantines, and not having persevered so long as yellow fever prevailed in the State, in its efforts to destroy or prevent it. The thing is possible; the expense should be counted as nothing; the means are simple and ample. We venture to point out a mode which seems best calculated for attaining the ends desired.

There should be appointed a commission of two or more gentlemen, of known scientific attainments and common sense, whose duty should be to collect all the facts relative to the nature and causes of yellow fever wherever it exists, here and elsewhere; to inquire into the hygienic measures which have been adopted to arrest or prevent it, whether successful or not; into the changes which have taken place in the cities and face of the country where it has ceased, and where it has but lately appeared. This commission should have money placed at its disposal to defray the expenses attended on these collections of facts, and if necessary, one or all the members should be enabled to visit places where valuable facts might be gathered, which could not be gathered without their personal attention. It should be made its duty to arrange the facts when gathered in a clear and condensed form to be laid, together with the plan for hygienic measures which the evidence showed best adapted for our locality, before the Executive and Legislature for their instruction and action. If it were possible to obtain a co-operation from the States of Alabama, Mississippi and Texas, it would be best to form a joint commission of one or more from each State. In this way, the expense would be divided, and the good effects of the result increased.

Moreover, these commissioners should be liberally paid. Our government has already shown itself "penny wise and pound foolish" in many matters besides



of the Public Health, and it is time to put a stop to that policy. It must be evident to all, that it is impossible for any men we have among us of very scientific attainments and common sense, to undertake such a labor and expense as we have shown necessary, depending upon their private means; and it would be unreasonable to expect them to leave their business and devote all their time to this matter without sufficient compensation. What the State has to do is absolutely to purchase these men body and mind, until their work is fully done; and, to use a homely adage, A superior article *ought* to cost high. We know gentlemen of the proper attainments and sense who have already devoted labor and money for years upon this subject, but whose progress has been slow and limited for want of means. Besides, whatever it cost, it has to be done, or the State has still to remain in criminal neglect of its duty.

If any method as effectual and less expensive than this we have pointed out can be devised, we would prefer its adoption. We can think of none. To do nothing has already cost, and will continue each year to cost infinitely more. To experiment without sufficient data, or rather, "go it blind," as legislatures are fond of doing, would surely cost more, and a thousand chances to one do no good.

Finally, we think the present system of quarantine, which is intrinsically good should be rigidly kept up against all infectious diseases all the year round. If the theory adopted by some with regard to yellow fever be true, a case imported into the city in March, as frequently happens, would be as effectual for its ultimate spread, as if it came in July or August. The heaviest expense has already been incurred, and to abandon the quarantine now would be ill advised in every way. The experience of ages has thought quarantine to be effectual against infectious diseases. To say from the partial experiment of one year, that it would not be so against yellow fever, would be too hasty a decision upon so vital a matter.

It may be said that our strictures and our—what shall we call it?—advice—should rather be directed to our city corporation. If our corporation will lend its aid, or if it will act in the matter for itself, it would be well. But this has become a State affair. There is not a town in it but is liable to be visited by the fever; and whether it be true that it originates in the city and towns, or that it must be imported into them, it certainly is true that it cannot become epidemic in either, unless the atmosphere or medium—whatever it may be—exists there in a fit state for its spread. Now, it is this very root of the matter which has to be found out, and when it is discovered, or whether discovered or not,—whether it be carbonic acid gas, or filth, or too little filth, or animalculæ, or cryptogamian plants, or whatsoever it may be,—when the means of preventing or destroying it are discovered, not only the city but the whole State will be benefitted. Then both may go rejoicing along a prosperous path, receiving to the full those blessings their soil, climate, commercial advantages, and the character of their people entitle them reasonably to expect.

VACCINATION.—By reference to our advertisements it will be seen that Dr. Fennell will, as usual, keep constantly on hand a supply of fresh and genuine Vaccine matter. In the absence of any State or City institution for supplying this long

established preventive of a dangerous disease, that is annually imported into this city, and from here too often conveyed to the interior of the country, we are glad to see the matter attended to by one so fully competent and reliable as Dr. Fenner. We will take this occasion to say, too, that *re-vaccination* is too often neglected of late, since this loathsome disease has evidently shown a greater tendency to prevail than was observed some years back. A very trifling expense will enable every person to be perfectly secure against an attack of it, and we would advise every one who has not been well vaccinated within the last ten years to have it repeated.

### CHARITY HOSPITAL.

The diseases most prevalent at this time are intermittent fever and bowel affections. Cases of the former are very numerous. Yellow fever has almost entirely disappeared. One case originated in the Eye Wards on the 27th ult. The patient had been in the hospital all the summer; he recovered.

In the Surgical Wards three cases of aneurism have lately occurred, all of which were of an interesting character. The first, of the femoral artery, in a woman. She attributed the origin of the tumor to a blow which she received from her husband. This probably had no part in its production. The tumor was larger than a man's fist, situated nearly mid-way down the thigh. The femoral artery was tied immediately below the profunda, at which point it was much diseased and dilated, being of sufficient capacity to permit of the introduction of the little finger. The whole aneurismal tumor, together with a small portion of the artery above and below it, was excised and the artery below tied. The patient recovered without a bad symptom and left the hospital five or six weeks after the operation.

The second, a diffuse aneurism of the popliteal artery, the result of a wound. Patient had been treated in private practice by compresssion without good result. Ligation was made at the point where the femoral artery pierces the tendon of the adductor magnus. The patient was convalescent two weeks after operation.

The above cases will be reported more fully in an ensuing number of the journal.

The third, aneurism of the profunda femoris. The patient was in the hospital three weeks and died. On post mortem examination, an enormous tumor was found occupying the lower portion of the abdomen and anterior face of the thigh of the right side, and extending from as high as the anterior superior spinous process to mid-way in the thigh. The aneurism was in part diffuse and in part saciform. The periosteum anterior portion of the femur and internal face of the ilium was destroyed, and the bones rough and denuded. The iliac, external iliac, and a part of the femoral arteries, upon which the tumor rested, were but slightly diseased. The profunda femoris, immediately at its origin from the femoral, was enlarged sufficiently to admit of the introduction of the index finger, but suddenly increased in capacity and formed a vast cavity, filled with coagula. The aorta presented several dilatations at different points of its course. The heart was healthy, with the exception of slight hypertrophy of the left ventricle. The radial and some other arteries were greatly ossified.

The patient had previously been operated on for popliteal aneurism of the left leg, by ligature of the femoral artery. This on examination was found to be completely pervious and healthy up to the point of the ligature.

# HEALTH OF OUR CITY.

*List of Mortality for Five Weeks, ending Oct. 31, 1855.*

	1st w'k.	2d w'k.	3d w'k.	4th w'k.	5th w'k.	Total.
Total number of Deaths,.....	181	152	121	121	106	681
Males, .....	120	112	81	70	67	450
Females, .....	54	33	33	51	32	203
Sex not stated,.....	7	7	7	0	7	28
Whites,.....	163	130	108	101	94	596
Blacks,.....	10	10	7	12	8	47
Mulattos, .....	8	8	6	7	4	33
Color not stated, .....	0	4	0	1	0	5
Native Americans,.....	39	27	27	30	25	148
Northern States,.....	5	3	3	3	6	20
Western States,.....	2	1	2	3	2	10
Southern States, .....	32	23	22	24	17	118
Foreigners, .....	78	82	48	45	31	284
English, .....	2	2	0	0	1	5
Irish,.....	27	32	19	19	11	108
French, .....	13	14	11	11	5	54
Germans,.....	32	21	9	9	9	80
Place of Birth not stated,.....	64	43	46	46	50	249
Age not stated,.....	21	11	7	5	16	60
Under one month old,.....	16	11	19	7	15	68
From one to five years,.....	23	26	18	30	17	114
From five to ten years,.....	3	2	1	3	1	10
From ten to twenty years,.....	17	10	7	13	3	50
From twenty to thirty years,.....	46	37	26	22	16	147
From thirty to forty years,.....	30	30	24	12	19	115
From forty to fifty years,.....	14	12	17	11	7	61
From fifty to sixty years,.....	6	6	1	7	7	27
From sixty to seventy years,.....	2	0	0	5	4	11
From seventy to eighty years,.....	1	6	1	2	0	10
From eighty to ninety years,.....	1	0	0	2	1	4
Over ninety years,.....	1	1	0	2	0	4
Yellow Fever,.....	70	46	24	15	11	166
Other Diseases, .....	94	85	83	92	81	435
Typhoid Fever,.....	3	5	2	9	3	22
Cholera, .....	2	4	2	0	1	9
Intemperance, .....	0	1	0	0	0	1
Accidental, .....	4	2	2	1	1	10
Still-Born, .....	8	7	7	2	6	30
Diseases not stated,.....	0	2	1	2	3	8

## CHARITY HOSPITAL REPORT — For the month of October, 1855.

Admitted, - - - - -	969
Discharged, - - - - -	772
Died, - - - - -	185
Births, — Males, 3; Females, 6; Still-born, 1. Total, 10.	

A DICTIONARY OF TERMS USED IN MEDICINE AND THE COLLATERAL SCIENCES.---  
By Richard D. Hoblyn, A. M.. Revised by Isaac Hays, M. D.—For the work, of which the above is the title, we are indebted to the publishers, Blanchard & Lea, through Messrs. J. B. Steel & Co. It consists, in a concise form, of the terms most used in medicine, with numerous additions by the American editor. The work has already come into very general use amongst students, on account of its low price, and we heartily recommend it to the profession as being both a convenient and correct book of reference

A MANUAL OF CLINICAL MEDICINE AND PHYSICAL DIAGNOSIS. By T. H. Tanner, M. D., *Licentiate of the Royal College of Physicians; Physician to the Hospital for Women, etc.*; to which is added the *Code of Ethics of the American Medical Association*. Philadelphia: Blanchard & Lea, 1855.—We have received from the publishers, through J. B. Steel & Co., of this city, the above entitled work. As a Manual, we can very confidently recommend it to the student, as well to such of the younger members of the profession who may feel the necessity of a work to which they may refer at any moment, to revive or test the correctness of their knowledge of the proper means for the investigation of disease

## Excerpta.

ANATOMY AND PHYSIOLOGY.—*Remarkable Case of Congenital Absence of Sternum; the Movements of the Heart Visible*.—An instance of congenital fissure, or the absence of the sternum, in a young man, a foreigner, excited very considerable attention this week at Guy's Hospital. We have seldom, if ever, seen a case of such intense interest, in a physiological point of view, as connected with the very vexed question of the sounds of the heart. The region of the sternum, in this individual, is, in fact, quite open, and displays under the skin the particular movements of the pericardium and heart. This fault of conformation interferes rather singularly with the actions usually ascribed to the sterno-hyoid and sterno-thyroid muscles, which are entirely absent at the right side, and very imperfectly developed at the left. The sterno-cleido-mastoid muscles, curiously enough, in their movements, extensively influence the neck, without a trace of sternal attachment; and in fact, when forced inspiration is tried, it is quite remarkable how the chest is expanded by these muscles, and the trapezius at each side, the diaphragm and the heart not seeming to be much affected, while the lungs, or what may be conceived to be a mass of pleura, mediastinum, and lung, being forced out, fills up the front of the neck. The man is now twenty-five years of age, and enjoys apparently good health. He has been examined by Skoda, Rokitansky, Bouillaud, Bernard, Piorry, Malgaigne, and some others on the continent, and has excited no ordinary speculation as to the sounds of the heart. He has been at Guy's Hospital, under the observation of Dr. Wilks, Dr. Parry, Dr. Addison, at St. Thomas's Hospital, under that of Mr. Simon, Dr. Peacock, etc., at King's College Hospital, under that of Dr. Todd, etc. We have been particularly struck, at a hasty glance of the case, by the



beautiful vermicular movement of the heart, by which the right auricle is filled with blood; for it does not appear to be the aorta which is under the skin, as at first suspected, but an oblong tumor covered with pericardium, the pulsations of which are not exactly synchronous with the pulse at the wrist, though it requires considerable care to detect the difference — more especially if the young man, the subject of the malformation, is tired by too much examination of the parts. This oblong tumor is most probable the right auricle, dilating and contracting, in a peculiar vermicular way, sixty or seventy times in a minute, and being about the size of two large thumbs of an adult, but made once and a half as large by the young man “holding his breath,” and thus allowing the auricle to be engorged. The diastole and systole are alternate, but not of that steam-engine piston character, up and down, as is usually thought; the period of the dilatation is longer than that of the contraction of the tumor or auricle; dilatation is, however, rapid, with an apparent tilt of the heart forward, immediately followed by a peculiar vermicular collapse, or emptying of the tumor, impeded only by “holding the breath,” or offering an obstacle to the blood going to the lungs. These various points are clear, and what, theoretically, might be expected. Not so, however, the relation of the sounds of the heart to the movements of systole and diastole; on which it is curious to perceive (looking through the note-book the young man takes with him) that all the first observers in Europe seem to have had peculiar and not very harmonizing opinions, from those offered by Rokitansky, Skoda, Bouillaud, Hamernik, Beclard, etc., down to the plessimetric crudities of Piorry, and the fancies of some American friends. The second sound is, perhaps, more marked than the first. It is difficult to arrive at any certainty on this and other points, however, as at Guy’s Hospital, on Tuesday last, every pupil in the theatre seemed anxious about some one other particular point, no little confusion necessarily ensuing. The two sounds are about equal in duration and force, but passing obliquely towards the base of the heart. The second sound (shall we call it valvular?) is more marked over the pulmonary artery, and to the ear even more superficial than that of the aorta. There did not seem to us any doubt that the impulse of the heart or tumor coincided exactly, not with the pulse at the wrist, as a ventricular phenomenon, but rather with the emptying of the subcutaneous tumor or auricle. We thought we could detect the systole of the right ventricle, agreeing with the phenomena of the opposite ventricle and pulse; a very appreciable interval, it must be remembered, exists between the latter and the exposed right *auricle*.

The movement of the tumor is very marked, as already stated, especially on forced expiration, or “holding the breath.” It (the tumor) sometimes appears fluttering or incomplete in its contractions or movements, pointing still further to its auricular character. The shock or impulse, so much a matter of controversy at the British Association and elsewhere, would appear to arise from the diastole of the ventricle, and not the systole, as usually believed. The second sound of the heart is, however, not so well cleared up by the case as the first sound. The second sound is now universally considered to be caused by the *click* of the valves, as suggested to us by Dr. Sieveking, who also examined the case this week. This fact is arrived at, of course, from pathological indications. It will be found interesting to confine the observations made on this young man (E. A. Graux,) as much as possible to the first sound, due so much to the impulse or shock of the heart, and synchronous contraction of both ventricles. — *Lancet*, June 23, 1855.

[We subjoin some further particulars of this very interesting and remarkable case from the *Gazette des Hôpitaux*, the patient having visited Paris, and been examined by the physiologists of that capital. The man proposes, we learn, to shortly come to this country to exhibit himself here.]

Alexander Graux, 25 years of age, presented himself before M. ARAN, exhibiting a malformation of the sternum, consisting in an almost entire division of that bone along its mesial line.

In the whole of the region naturally occupied by the sternum there existed a depression or gutter-like cleft, its walls formed by the integument only. This depression was about three and a half inches in length, measuring it vertically from a line drawn from one clavicle to the other, and assumed a triangular shape, the base above in the space between the two clavicles, and the apex below. At its

base it measured about two-thirds of an inch, and at its apex about an eighth. The dimensions of this furrow could be considerably increased at the pleasure of the patient by putting his pectoral muscles into strong action, the integument covering it being then stretched so as to occupy the same level as the rest of the wall of the thorax, the depression being entirely effaced.

When in a state of repose, an osseous rim, evidently formed by the two halves of the sternum, was distinctly seen on either side of the cleft, and to this the ribs were connected in the usual manner. This malformation entailed some considerable modification in the relations of the osseous and muscular structures of the neck and chest. The clavicles, natural in their curve and structure, terminate in a large extremity above either half of the sternum, without any trace of a sterno-clavicular articulation. The sterno-cleido mastoid muscles arise from the clavicle only; properly speaking they are cleido-mastoid, the right sterno-hyoid and thyroid are absent, but on the left side they can be seen during a deep inspiration, resembling two prominent cords. The respiratory functions are no less considerably modified by this malformation. An ordinary inspiration is at once diaphragmatic and superior costal, with this peculiarity, that the diaphragm acts first, the upper ribs rising after the commencement of the recoil of the abdominal walls. During a deep inspiration the chest is raised and enlarged by the trapezious and sterno-mastoid on either side, while there is no proportionate increase in the action of the diaphragm and upper ribs. There is nothing remarkable in an ordinary inspiration, but during a forced one the chest and the abdomen retract, the veins of the neck become conspicuous, some of the intercostal spaces become convex on the outer surface, and then the fissure commences to rise from its middle upwards until the integument becomes level with, or even projects beyond the level of the neighboring structures.

Owing to this malformation, the mode of performing some of the functions of the heart can be studied through the thin covering of integument. About the middle of the fissure an oblong tumor alternately dilates and contracts from sixty to sixty-four times in the minute. When most dilated, the tumor is about two inches in its greatest or vertical diameter, and it diminishes from one-half to two-thirds when contracting from above downwards; and from the right to the left side, the dilating and the contracting alternate, and the former occupies rather more time than the latter. This pulsating tumor is relatively less resonant on percussion than the neighboring parts, and this dullness is continuous with that of the ventricles, or of the heart properly so called, measuring three inches from side to side, reckoning from the mesial line, and three and a half from above downwards. At the level of the tumor the two sounds of the heart are very loud and clear, especially the second. At the boundary of the ventricular dullness, which extends beyond the level of the impulse, the two are equally loud, clear, and nearly of the same duration; but ascending obliquely towards the base, the second sound becomes stronger and clearer, and near the sternum the first acquires a blowing character. The second sound derived from the pulmonary artery is more distinct and superficial than that of the aorta. When the hand is placed upon the spot at which the impulse is most distinctly felt, viz: in the fourth intercostal space close to the nipple, when the patient is lying down, or a little below the fifth rib if he is standing, this impulse is found exactly to coincide with the commencement of the contraction of the tumor. If the fingers are placed on the radial or the carotid while the eye attentively watches the movements of the subcutaneous tumor, an appreciable interval is noticed between its contraction and the dilating of the artery, the latter being always consecutive.

Such are the principal peculiarities presented by this malformation. The young man enjoys excellent health, though of a feeble appearance. For the present we record the phenomenon without seeking to deduce from it any argument in reference to the many different theories respecting the movements of the heart, and we abstain from bringing forward the diverse explanations which have been already offered.

A commission composed of MM. Aran, Beau, Béheir, Bouvier, Hérard and Monneret, has been requested to report upon the subject to the Medical Society of the Hospitals. — *American Journal*.

**SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY. — ON CICATRICES.** *By M. Jobert.*—A cicatrix is a kind of protective texture, intended to close in solutions of continuity, and to cover over organic tissues in the absence of the natural integument. When the membrane which covers the granulations has been formed, the remarkable retractile power exerted by cicatricial tissue is brought into operation, which, by the centripetal action, constantly diminishes the extent of the wound and the vascularity of the newly-formed product. This retraction which is greater in proportion as suppuration has been prolonged, persists long after the closure of the wound.

If we examine a cicatrix that is completely formed, we find that it consists in : 1. A thin superficial membrane which, though sometimes rugous, is usually smooth, shining, and, and from the absence of secretion, dry. This protects the cicatricial or inodular tissue, just as the epidermis protects the parts beneath it. 2. Below this is a peculiar tissue, consisting of a mass of irregularly interlaced and very resistant fibres. It supplies the place of the dermis, is the fundamental portion of the cicatrix, and may be compared to ligamentous tissue. 3. Arteries and veins, small in number and in size, are found in the centre and midst of this substance. 4. Although the existence of lymphatics has not been demonstrated, it is highly probable that they accompany the blood vessels. 5. The careful dissection of a great number of cicatrices has never shown M. Jobert the slightest trace of nerves; but he has always found the nerves of the surrounding parts terminating at the periphery of the cicatrix in little gangliform swellings, exactly analogous to those found at the end of stumps after amputation. This enables us to explain the complete insensibility of the cicatricial tissue itself, and the keen sensibility of the integuments at its periphery. Moreover, if we detach a flap from the vicinity of the cicatrix, attach this to its centre, and, when union is effected, divide the pedicle, we find that this islet of integument retains its sensibility in the midst of a tissue quite insensible to external impressions; the nervous influence seeming, as it were, to traverse the cicatricial tissue as a conductor to reach the cerebro-spinal axis.

This inodular tissue is liable to undergo alterations. We frequently find, during changes of temperature, etc., cicatrices liable to pruritus; the patients scratching them until redness or excoriation is produced, without obtaining the slightest relief. Such irritation may lead to the belief that the cicatrix is very sensitive; but on examination we find it has its seat in the nervous innervations at the periphery. The integuments may also have been at first incompletely destroyed, and any portion that has so escaped may retain its sensibility under the cicatrix. The cicatrix may appear red and swollen at the menstrual period or under certain atmospheric conditions; but such changes rarely take place in the congested cellular tissue and hypertrophied fatty tissue beneath the cicatrix. If the texture of cicatrix be not firm enough to resist the traction exerted upon it, it may become torn or ulcerated. Ulceration, too, may spontaneously arise, and reparation is effected by means of new inodular tissue, which renders the cicatrix firmer than before. Sometimes a cicatrix is invaded by diphtheritis, this being frequently the case in the vagina. Occasionally, but much more rarely, mortification of the tissue takes place.

It is to the retractibility of this tissue that the production of the deformities commonly called "bridles" is due; and the surgeon should, by his prophylactic treatment, earnestly endeavor to prevent the formation of vicious cicatrices. To this end he should endeavor to diminish the suppurative process, repress too luxuriant granulations by compression or cauterization, moderate the inflammatory process by refrigeration, and place the part in a position opposed to the force exerted by the inodular tissue, so that the lips of the wound may diverge as far as possible. To these points he should direct all his attention, for after a vicious cicatrix has been once formed no mechanical appliance can ever disengage it, and to restore to the different regions their freedom of action we must have recourse to an operation. But we must take care not to operate in every case of deformed cicatrix, the prudent surgeon abstaining when the deformity is too considerable to be cured. We must not operate either when the cicatrix is too recent; for its vitality and vascularity being but little developed, it may abscess or mortify.



while the inodular retractility it still preserves, prevents the success of the operation, by incessantly drawing upon, and destroying the adhesions that may form. There are two modes of remedying these deformities. The old one, which, indeed, is not yet abandoned, consists in completely dividing the cicatricial tissue, retaining the lips of the wound separated as far as possible, and healing by a new granulation. But if the cicatrix has contracted adhesions, the two lips can hardly be separated, and the operation proves useless. Yet, by this means, M. Velpeau has recently removed a very considerable bridle with success. Delpech was the inventor of the new procedure. He circumscribed the entire cicatrix within two semi-elliptical incisions, dissected it out, and united the healthy tissues by means of sutures. This, very well suited for a cicatrix of small extent, cannot be applied to a very large one, even when aided by the lateral *debridements* and detachment of the integuments recommended by Delpech, for the purpose of preventing traction, and for repairing the loss of substance produced by the bistoury.

M. Jobert operates by what he terms a mixed procedure, as follows: He makes a transverse incision through the most prominent part of the bridle, avoiding the subjacent vessels and nerves. Next, forming from the vicinity a flap sufficiently large to restore to the part the elasticity of its movements, and thick enough to be sufficiently vascular, he twists it on his pedicle, and applies it by means of the interrupted suture between the separated lips of the incision. When it has firmly united there, the pedicle is divided. In this way the motions of the part become quite free, and the deformity cannot be reproduced.—*Med. Times and Gaz.*, Aug. 11, 1855, from *Gaz. des Hôpitaux*, No. 63.

ON THE OCCURRENCE OF CANCER IN CHILDREN. *By M. Guersant.*—The subject of cancer in children is nowhere treated of, probably on account of the rarity of this affection during the early periods of life; however, there is not a year in which the author does not see some cases of it.

For a very long time, cancer was met in children only in the orbital cavity, whether it took its origin from the globe of the eye or the base of the orbit. It has since been proved to occur in the testicles. Dupuytren operated on several such cases; M. Guersant has also removed six or seven testicles affected with encephaloid cancer. Cancer has also been found in other parts, but less frequently, unless in the vulva. Ought we now to make a distinction between fibro-plastic and cancerous tumors? M. Guersant is strongly disposed to class them together, on account, he says, of the tendency the form possess, in common with the latter, to relapse. He admits, however, that their relapse occurs less frequently; and in this point of view they have a character of relative benignity which should be borne in mind, but they are nevertheless, according to this surgeon, closely allied to cancer.

The progress of a cancerous affection in a child is much more rapid than at a more advanced age. At Salpêtrière and Bicêtre, cancer is seen in old patients to remain stationary for from fifteen to twenty years. In children, on the contrary, the progress of the disease is terrific. In a young boy, under M. Guersant's care for a cancerous tumor in the orbital cavity, the eye was driven out of the orbit in less than six weeks. The sufferings of this little patient were fearful; the globe of the eye was extirpated, but the disease returned with extreme rapidity, and at the end of a month the ethmoid was destroyed.

The prognosis in cancer in children is, therefore, very unfavorable; and because there is no chance of the disease remaining stationary, the indication is to operate with more rapidity and energy than in adults. But, on the other hand, relapse is so general, and occurs so quickly, that we ought to resolve on operating only with the view of procuring temporary relief from intolerable sufferings; nor should we fail to warn the friends of the patient of the likelihood of the disease returning.

M. Guersant quotes, in support of these remarks, a case which terminated fatally; although, on *post-mortem* examination, nothing but fibro-plastic tissue was found in the tumor, the destruction of which had been attempted. The patient had been a little girl, aged thirteen months, who had a tumor in the vulva. This tumor was tied by a surgeon on the 6th of November, and came away. The disease relapsed, and on the 18th of December a second ligature was applied: re-



lapse occurred still more speedily than on the first occasion. In January, the second tumor was pretty large, and its presence gave rise to sympathetic vomiting. The child was presented to the Surgical Society, the members of which body verified the existence of a multilobular tumor, with prolongation into the vagina. Although it was difficult to trace its limits, M. Huguier advised removal with the bistoury, and cauterization of its roots. MM. Lenoir and Guersant were of opinion that the results of such an operation would be incomplete. However, the tumor mortified, and the child fell into a state of marasmus. M. Guersant thought it advisable once more to have recourse to ligature; but the little patient was already exhausted, and sank in a few days from adynamia.

Should excision have been more successful? It is not probable. Nevertheless in another case, M. Guersant, considering such an affection as a sort of polypus, would excise it early, and cauterize its base with liquid caustics; and if the vagina was large enough to admit of these means, he would not despair of obtaining a more or less permanent cure.—*Dublin Medical Press*, July 25, 1855, from *Journal de Méd. et de Chirur. Pratiques*, July, 1855, p. 299.

ON DR. LANDOLFI'S TREATMENT OF CANCER. — *By M. Lasèque.* — Dr. Landolfi, surgeon-in-chief of the Sicilian army, and lecturer on cancerous diseases at the Trinity Hospital, Naples, is now visiting different parts of the Continent, for the purpose of propagating his method of curing cancer. Having secured many adherents at Vienna, he has repaired to Paris, where a certain number of patients, selected from Salpêtrière, have been placed under his care, a medical commission watching the results. As this method, though exciting much attention in Italy and Germany, is scarcely known in France, and we may add in England, M. Lasèque, while awaiting the report of the commission, proceeds to give some account of it: and although naturally prejudiced against any specific method of treating the disease, the above-board course of procedure adopted by Landolfi, and the large amount of testimony of success he adduces, have evidently made a considerable impression upon him.

The specific employed by Landolfi is the chloride of bromine, applied externally as a caustic, and administered internally, the latter being of quite secondary importance. This caustic, mixed into a thick paste with liquorice powder, may be employed alone, or it may be combined with other caustics, as in the following formula: R. — Chlor. brom. three parts, chlor. zinc. two parts, chlor. antimon., chlor. auri. ana, one part. To be mixed in the air, on account of the fumes disengaged. In open cancer, Landolfi regards the chlor. zinc. as indispensable as a hæmostatic; and the chlor. auri. seems to exert a special action in encephaloid. Cutaneous cancer, epithelioma, lupus, and small cystosarcomas may be treated by an ointment formed of one part of chloride of bromine to eight of basilicon. The healthy parts around the tumor are to be protected by bands of linen one and a half to two inches broad, covered with ointment (four parts of chloroform to thirty of lard,) and the patient is placed near a window, so that the fumes may escape. Small compresses, upon which the paste has been spread, are gently applied to the part, in an imbricated manner, so as to secure exact juxtaposition, keeping two lines clear of the sound parts. The whole is then covered with charpie and diachylon. A sharp burning sensation is soon followed by severe pain, which may last for several hours, and is combated by repeated doses of anodynes. The paste is usually kept on for twenty-four hours, and on its removal a line of demarcation is generally perceived. The tumor is in part white, and in part reddish, or mottled with yellow and blue. Bread or lettuce-leave poultices, or basilicon ointment, are now applied every three hours. As the gangrene proceeds, the pain diminishes, and about the fourth or fifth day the eschar loosens, being removable by the forceps from the eighth to the fifteenth. A healthy granulating surface is then exposed, and if any vestige of the disease is observable, a little paste is reapplied to that spot. The wound is now to be treated as a simple ulcer, and if there is a deficiency of suppuration a lotion is to be applied, containing from twenty to thirty drops of the chloride of bromine in five hundred grammes of Goulard water. The ulcer usually heals rapidly from the circumference, the cicatrix resembling that resulting from incised wounds.

In spite of the severe pain, there is rarely febrile reaction, and no change in the patient's regimen is required. A. Leugr Landolf believes the paste acts by absorption, as well as locally, and regards internal treatment as only adjuvatory, and only so employs it, in the hopes of preventing relapse. The treatment, even in very bad cases, usually at least affords very notable relief, and in such as are quite hopeless or inaccessible to caustic, a lotion, consisting ten to twenty drops in five hundred grammes of water, may exercise some useful mollifying power. — *B. and F. Med.-Chirurg. Rev.*, July, 1855, from *Archives Générales*, May, 1855.

**REDUCTION OF THE LOWER JAW REDUCED BY A NEW METHOD.** — *By W. C. M.* — Miss ———, æt. 25, while in bed, fell in a protracted puerperal. Felt a jerk in the jaw, and found she could not close her mouth. She immediately applied to a medical man in her neighborhood, who recognized a dislocation, but failed in his efforts at reduction. She then applied to Mr. Collins. "The mouth was open, the symphysis of the jaws slightly protruding. On applying the fingers to the angle of the jaw, and tracing the manus upwards, it led in a direction much anterior to the position of the articulating surface. The posterior edge of the bone could be most distinctly felt, and a broad furrow or hollow existed between the bone and the ear. The heads of the bone were felt and perceived prominent in front of their natural position, so that the face appeared broader at this place than natural."

"Before attempting reduction, I wished to ascertain the position in which I would have most command of the force to be used. Standing before her, I passed both thumbs into the mouth, but felt I would not have a position the most favorable for applying all my force, if necessary."

"I then stood behind her, and it at once struck me this was the position which afforded most advantages."

"Placing her head against my chest, I passed each thumb as far back on the corresponding side of the jaw as possible. By making a rotatory motion from the wrist, I found the bone to yield: by now adding a motion of drawing the hand in towards the chest, the left side first, then the right, slipped into their positions, and the patient closed the mouth, the rows of teeth falling into their relative positions, and she now could speak plainly."

"I think there are many advantages to be derived from attempting reduction in this posture, viz: the surgeon standing behind the patient, the head applied to his breast, and the thumbs turned inwards on the corresponding angles of the jaw, the fingers under the bone in front."

"In the first place, the head is much more secure than in the original process, where it is applied against a wall, because in the latter the surgeon may press down the bone, and the patient generally will draw the head in the same direction by moving the body forward in a chair."

"By standing behind the patient, while depressing and pushing back the thumbs, he is pressing forwards with the chest, and thus fixes the head more steadily, and assists his manipulations; and even if the patient do move on the chair, a slight motion of his body will suffice to counteract this movement, and retain the head steadily fixed."

"Another advantage is, that he can use much more force, because when standing in front he can only use the muscles that depress the hands; whereas, standing behind the patient he has the power of those muscles, and is assisted by the powerful class of muscles that rotate the thumbs inwardly; and, besides, in the former case, his pressure is away from his body, whereas, in the new opposition the pressure is more directly downwards and towards himself. The only disadvantage in this proceeding, if it can be considered one, is, that the mouth is stretched more than in the original plan." — *Dublin Hospital Gazette*.

**OPACITY OF THE CORNEA TREATED BY OPERATION.** — Dr. M. DAVIS reports (*Med. Times and Gaz.*, Aug. 18th last) the following cases of opacity of the cornea treated by operation under the care of Mr. HAYNES WALTON and Dr. TAYLOR. A man, about 50 years of age, a patient of Dr. Taylor's, had a quantity of lime thrown into his eye four years ago. The eye was immediately washed out, and it was supposed that all the foreign matter had been removed, but a dense white opacity re-

remained, covering nearly two-thirds of the cornea, and completely concealing the pupil when in a state medium of contraction. Many ineffectual attempts had been made to remove or diminish the opacity, at first by means of lotions and other local applications. On examining the eye minutely, it was seen that the opacity was smooth and uniformly covered by the epithelium; its upper edge, where it did not extend to the margin of the cornea, was elevated irregularly, and the surface generally presented a very marked elevation of the upper part of the cornea. This elevation, taken in connection with the history of the case, led Dr. Taylor to suspect that the opacity might be a remnant of a portion of the time which had not been removed at the time of the accident, and had become incorporated with the corneal tissue. He therefore, with the aid of his knife, carefully raised the epithelium in front of the pupil, and found that, by careful manipulation, the opacity could be peeled off, and it was found that in no part, towards the centre of the cornea, did it appear to have penetrated the anterior elastic lamina. After clearing the pupil, the operation was discontinued for the time, partly on account of the severe pain which it occasioned, and partly to avoid the risk of inflammation. On a subsequent occasion, the remainder was removed, with the exception of a few small spots towards the margin of the cornea, which appeared to be due to interstitial inflammation of the cornea.

The slight haziness which remained after the operation was speedily dissipated, and the man was dismissed with almost perfect vision. Chemical examination showed the opaque matter to consist of carbonate of lime.

In another case, also under the care of Dr. Taylor, the opacity was removed, partly by operation, and partly by the process of absorption, excited by mechanical irritation.

The patient, a female, 24 years of age, had been subject, all within the last six years, to attacks of ulceration of the cornea. She now applied on account of a central milky opacity of the right cornea, shading the pupil and destroying useful vision in the eye. It had remained undiminished in size for six years, notwithstanding a great variety of local applications. Near the centre of the opacity were two small, dark-brown spots, situated, apparently, in the substance of the cornea. These were, probably, the effects of a former long-continued use of nitrate of silver solution, while the cornea was ulcerated. The surface of the opacity was readily peeled off in small flakes, by a cautious use of the iris knife, but the brown spots were found to be so deep-seated, that Dr. Taylor did not consider it prudent to interfere with them, especially as they would not impede vision. The result of this little operation, which has since been repeated, has been the rapid diminution of the opacity, and corresponding improvement in vision; and as absorption is still going on steadily, there is every prospect that the sight of the eye will be completely restored.

It might be objected, that the following case ought not, in strictness, to be placed under the heading of this report, but it is given, not only on account of its great peculiarity, but because it is somewhat allied to the above.

T. P., aged 38, a meteorological instrument-maker, discovered about four years ago, that the left eye was misty. The mistiness increased slowly, and attributing the failing of sight to the injurious effects of his trade, he disregarded professional advice, till the right eye had given evidence of the same kind of obscurity that had attacked its fellow, and now he applied to Mr. Walton. The eye first diseased, the left, is virtually blind, for nothing can be seen with it, as in the centre of the cornea there is a brown oval opacity, placed transversely, large enough to cover the pupil, and dense enough to intercept light. It is of a sepia color, and shaded towards the extremities, not raised, and possessed of the same lustre as other parts of the surface of the cornea.

The right eye is affected in a similar manner, but in a less degree, and enough of the pupil is yet uncovered, that with a magnifying glass the coarser works of his trade can be executed. There have not been any subjective symptoms, and he himself is quite unaware that there are brown spots on his eyes.

Mr. Walton directed atropine to be used to the left eye, the effect of which was to dilate the pupil beyond the opacity, and thus to enable objects to be seen with that eye nearly as well as with the other.



The right eye was then treated in the same manner, and the vision was improved. The patient now expressed himself quite satisfied with the benefit received, and desired to cease attendance, but yielded to the request of Mr. Walton to attend another day, that he might ascertain how far the opacities were capable of being removed by operation. An attempt was made to scrape a portion of one away, but a clear surface beneath could not be obtained, as the disease had extended into the true texture of the cornea, and perhaps completely pervaded it.

Dr. Taylor, who had taken his microscope to the hospital, to examine, in a fresh state, whatever might have been removed, found that the portion separated consisted of epithelium, some of which contained pigment granules.

I must beg to observe that, so far as my personal experience goes, opacities resulting from loss of substance of the cornea: in fact, cicatrices and interstitial deposits from inflammation are not capable of being pared away, but depositions, for the most part consisting of earthy materials on the surface of the cornea, and the accidental impingement of a foreign substance, as in the first case, may be so removed.

**TREATMENT OF CHOREA BY GYMNASTICS.**—*M. Blache* has submitted a memoir on this subject to the Academy of Sciences. His remarks are founded upon the experience of 108 cases, 84 girls and 24 boys, treated by gymnastics alone, or by these in combination with other measures, such as sulphureous baths, etc. In 102 of these cases the cure was completed, on an average, in 39 days: and in the 6 remaining, which he considers as unsuccessful instances, recovery took place in 122 days. The committee appointed by the Academy to examine *M. Blache's* paper, have reported on it in the most favorable terms, and considers the method of treatment advanced by him as deserving a place among the most approved curative means known in connection with this disease. The author has not only brought forward practical evidence of the successful employment of gymnastic exercises in chorea, but investigates their probable mode of action in that affection: and has fully established that the efficacy of this method of treatment equals, in most cases, that of any other mode with which we are acquainted, while the remedy is free from those objections to which many others are liable.—*Edinburgh Medical Journ.*, July, 1855, from *Gazette Méd. de Paris*.

**ON THE SALINE TREATMENT OF CHOLERA.**—By *A. Leckie, Esq., C. M., Glasgow*. From certain reports lately published I observe a decided inclination to underrate saline treatment of Asiatic cholera. It is true the writers, or the parties who got up these reports, speak of the great difficulty of finding reliable and satisfactory data on the comparative effects of the various modes of treatment; but when this is the case I think it very improper that imperfect deductions should be made, and given forth to the community as the results of scientific investigations.

I have a good deal of experience in cholera, especially during the last two visitations, and have given particular attention to the various modes of treatment which have at various times been adopted by medical practitioners. The more I see, either in my own practice or in the published experience of others, the more I am convinced that the saline treatment is immeasurably the best that has yet been adopted, and, if properly carried out, would bring the mortality as low as probably any method that may yet be discovered. During the last visitation I had twenty-one cases, the greater number of them being more or less collapsed, yet all of them recovered. I am anxious to impress the minds of practitioners with the fact that salines are efficacious, not only in cholera, but also in diarrhoea. As proof of this I have had within the last year above one hundred and twenty cases of diarrhoea, in not one of which was either opium or astringents given, but simply the salines with two or three grains of calomel, yet not one of them merged into cholera.

In regard to cholera, I have invariably found that if opium or stimulants had been given at any period of its progress, the disease was much more difficult to subdue. When such medicines are given in conjunction with the salines, no favorable effect can be expected; yet this is frequently done, and when fatal results ensue, the salines are pronounced a failure. They must be given alone, or the saline method



gets no trial at all. Two cases which had been thus drugged before I saw them, were the only two that suffered from consecutive fever. There is still another point which must be particularly attended to, and that is, to have faithful and vigilant nursing. In aggravated cases, unless great attention is given, occasionally varying the salines to suit the particular condition of the patient, and carrying on the external appliances with regularity and vigor, we cannot expect favorable results.

The medical practitioner must not rest contented with giving general instructions, he must make frequent visits, scrutinize with a jealous eye the conduct of the nurses, and occasionally set to work with his own hands. If these things be properly attended to, the happiest effects may be expected, even in what might appear to be hopeless cases.

In order to show this, and to give some idea of the attention that is necessary in cases of advanced collapse, I subjoin a few particulars in reference to one of my latest cases.

Mrs. L——, Bonhill, aged twenty-three, was seized with diarrhœa about two o'clock, A. M., on the 20th of September, and with vomiting and severe cramps about seven o'clock, after which she rapidly sank and became cold. Medical aid was sent for about eight o'clock, but could not be got till about one o'clock, P. M., when I visited her. I found her vomiting a fluid resembling rice-water; passing stools involuntarily; skin cold; face of a leaden hue, covered with a cold sweat; tongue cold and pale; breath cold; eyes sunk in their sockets, and turned upwards, surrounded by a dark areola: whispering voice; respiration very slow; pulse not perceptible; hands blue; urine suppressed. First passed urine about two o'clock, P. M., on the 23d; first bilious stool about four o'clock, A. M.

Sept. 24th.—Recovered.

Treatment, salines. She took about six dozen of saline powders of different kinds. I visited her every two or three hours.

The following is the method I generally adopted:

*For Diarrhœa.*—One of the saline powders to be taken in three-quarters of a tumbler of water; another two hours afterwards. Two grains of calomel to be taken two hours after the second saline powder. At the expiry of other two hours, take another saline powder, which is to be repeated every two hours till the diarrhœa abates.

*For Cholera.*—One of the saline powders to be taken every half-hour, hour, or two hours, according to the severity of the case. If the vomiting continues after taking three or four saline powders, half a drachm of the carbonate of soda, or a common effervescing soda powder, sometimes also a small piece of ice, should be given every half-hour, or hour, till the vomiting cease. Then again commence the saline powders, omitting the muriate of soda. Afterwards give them with the muriate of soda, and without, alternately.

When the patient is far advanced in the collapse stage, a tablespoonful of salt, in three gills of water, or beef-tea, should be thrown up the bowels every three hours.

Avoid, in all stages of the disease, the taking of brandy or stimulants of every kind, or opium in any form, as they are highly pernicious. In all cases, apply mustard poultices to the stomach and bowels.

If the extremities are cold, apply jars with hot water: and for cramps, friction.

Diet, arrowroot and beef tea.

Drink, cold water, common effervescing soda powders, or ice.—*London Lancet.*

VACCINATION OF DOGS.—We find in the *Deutsche Klinik*, a suggestion which may be of value to the lovers of dogs. In No. 7 of that journal for 1855, Dr. Sautlus states that he is acquainted with a sportsman enjoying some reputation as a trainer of dogs, who vaccinates all the pups on the nose. He contends that this operation entirely protects the animal from the distemper.—*Virginia Med. and Surg. Journal.*

**STONE IN THE BLADDER, COMPLICATED WITH STRICTURE OF THE URETHRA IN A PATIENT AGED EIGHTY-SIX. LITHOTRITY: RECOVERY.**—*By Francis W. Allen, Esq., M. R. C. S.*—The following case is worthy of notice, not only from its complications, but from its having occurred in perhaps the oldest patient on record on this country, upon whom the operation of lithotomy was most successfully performed. Thus, in 512 cases of lithotomy recorded in Mr. Coulson's *Work* on this subject, I find that (*op. cit.* pp. 878) from the age of

1 to 20 years there are	25 cases.
20 to 30	22 "
30 to 40	22 "
40 to 50	22 "
50 to 60	22 "
60 to 70	22 "
70 to 80	22 "
80 to 90	22 "
90 to 100	22 "

W. G.—has for the removal of a large elastic stone of the urethra, which has necessitated the introduction of the catheter, a No. 4 elastic being the largest which has been passed during that period. He has now been suffering for a long time with occasional diarrhoea, attended with constant and urgent desire to pass urine, which is loaded with mucus and pus, alkaline, and very offensive, and he describes himself as being in a most wretched and miserable state.

The diarrhoea and general debility being such, he was unable to pursue any measures, a small sound was passed, and a catheter introduced, which occupied a side of the bladder over its fundus.

Mr. Coulson, who had known the patient for many years, having been consulted, proposed an operation for the removal of the disease, intending to perform lithotomy or lithotripsy as might be practicable, after attempting to dilate the urethra, the success or non-success of which I was to communicate to him.

On April 7th I passed a No. 5 elastic catheter, and found one stricture about three inches from the meatus, and another in front of the bulbous portion of the urethra. These instruments were gradually increased in size daily, retaining them in the bladder for twenty minutes, until the urethra was sufficiently dilated to admit a No. 12 catheter.

26th.—Mr. Coulson performed the operation of lithotripsy in a most efficient manner. On account of some enlargement of the prostate, a large lithotrite (made by Weiss) was used, which having been introduced, the stone, about an inch in diameter, was seized and crushed in a few seconds. The lithotrite having been withdrawn, the bladder was washed out with warm water, and the patient retired to bed, without much inconvenience beyond a rigor, which debilitated him a good deal, but which was relieved by the prompt use of opium, brandy and water, hot bottles, &c.

17th.—The patient does well: some pieces of stone and detritus having been passed, mixed with mucus. Ordered a quarter of a grain of morphia at bed-time.

18th.—Continues well. Introduced a silver catheter, and washed the bladder out with tepid water, bringing away some grit, which relieved the patient much. He is down stairs. Has passed several pieces of stone, one of which remained in the urethra opposite one of the strictures for some hours: bowels confined. To take rhubarb draught.

19th.—Mr. Coulson visited the patient, and crushed some fragments, after which there was a slight rigor. To take a quarter of a grain of morphia immediately.

20th.—The patient is down stairs, and feels pretty well. Continue the pill. From this time out he has gradually recovered. The bladder has been washed out every other day with warm water, and there is now not a trace of grit: the urine is nearly clear of mucus. He has again his urine for several hours without inconvenience: has a good appetite, and thoroughly enjoys the amusement of his garden and carriage exercise. *Do. on 1st June.*

**CHOLERA.**—Dr. Varrentrapp of Frankfurt on the Main, after a careful examination of the various epidemics of cholera which have visited Germany, concludes that there is irrefragable evidence to show that the disease is contagious, and especially that excrementa, and the linen and other clothes when soiled, are capable of propagating the disease—*Virginia Med. and Surg. Journal.*

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Wound of the Palmer Arch.

By WARREN STONE, M. D., Prof. of Surgery, University of La.

In the September number of the *London Lancet*, an interesting case of wound of the palmer arch, is related by Mr. Skey, in which the radial and ulnar arteries were first tied, and subsequently the ulnar in the middle of the fore arm, the brachial at the usual place, and the axillary arteries. Hemorrhage occurred after each operation, and the case was finally saved by compression. Mr. Arnott, in an article published in the October number of the *Lancet*, criticises the above case with some severity, condemns the ligature, and relates a case in illustration and confirmation, of his views. The surgery of the arteries (as is generally practiced), is sufficiently bad, without additional authority to encourage the ignorant and timid to temporise and tamper with human life, when decisive measures would render the case safe or at least much more secure. In Mr. Skey's case, seventeen days had elapsed after the wound, and repeated hemorrhages had exhausted the patient and impoverished the blood, and from repeated and continued efforts to control the bleeding by compression, the wound was rendered unhealthy and the hand swollen and inflamed. The ligature was the proper remedy in this case beyond a doubt, and if compression had been judiciously made in support or in addition, the case would have recovered no doubt with the single operation of ligating the radial and ulnar arteries. Mr. Arnott's case was a first wound, and it recovered under the treatment by compression, as all such cases ought, and will, if properly dressed and properly managed afterwards; but if the dressings do not happen to be applied

with precision, or from some other cause bleeding should occur after some days, and either from too much pressure, or from some constitutional cause, the wound becomes unhealthy and inflamed, the case is quite different. It is quite possible, that, if Mr. Arnott, or some unequally skilled in the application of the bandage, could dress and attend all similar cases from the beginning, these bad cases would not occur; but this cannot be, and secondary hemorrhage will take place, complicated cases will occur, and surgeons will be called upon when compression has failed, and it is time that the duty of the surgeon should be defined in such cases. Mr. Arnott objects to ligating the radial and ulnar arteries, because it is dangerous, unnecessary and inefficient. I have tied these vessels many times for secondary bleeding from wounds in the hand, and always with success. In only one case has bleeding occurred at the wound, from the anastomosis of the interosseous artery, and then it was trifling. I believe that if the wound is properly dressed in the first place, secondary bleeding will not occur, at least it has never happened with me. But I have exercised all my skill by compression when secondary bleeding had taken place without success, and was obliged to resort to the ligature. Wounds of the planter arch are very similar to wounds of the palmer arch, and I have always treated them in a similar manner; that is, when secondary bleeding has occurred, I have tied the anterior and posterior tibial arteries, and always with success, with one exception. In this case, free hemorrhage took place from the wound three days after the arteries were tied, and I tied the femoral artery with success. The proper way to treat wounded arteries, is to tie them at the wound, if they are accessible; but if they cannot be tied without an operation, the surgeon is justified in trusting to compression, in all arteries of the size of the palmer arch and even larger; but if secondary hemorrhage takes place, there is danger in trusting to compression, a second time, though under some circumstances it may be justified. If the wound is healthy, there is a possibility that the mouth of the artery may be closed by granulations; and if the patient has not already suffered from the loss of blood, or is in condition to suffer another hemorrhage without danger, this chance may be given him, rather than resort to the ligature. But if the wound is unhealthy, and no granulations forming, it is wantonly jeopardizing the patient's life to trust longer to compression. Mr. Arnott urges that ligating arteries is not free from danger. I believe him, but I cannot believe that an artery, with a ligature properly applied to it, is so dangerous as the same artery divided and without a ligature, even with Mr. Ar-



nott present to make compression. I fully appreciate the value of compression, and believe if it was more used in addition to, and in aid of the ligature, second hemorrhage, or hemorrhage after the ligature comes away, would be rare. But I feel no particular interest in Mr. Arnott or Mr. Skey; neither of their articles are very instructive as they stand. Mr. Skey had a bad case, and endeavored to justify his extreme course, and Mr. Arnott seizes the occasion to make a comparison of his treatment in such cases; and the result in both cases is calculated to encourage too much reliance upon compression, or rather to encourage a reliance upon it in improper cases. Compression is effectual and sufficient in certain cases, and the ligature is positively necessary in certain other cases, and it is not difficult to determine when the ligature ought to be applied. The truth is, that it is not easy for one not accustomed to it, to apply a ligature to an artery, however well he may be theoretically informed in anatomy, and there is too great a disposition on the part of practitioners to allow repeated hemorrhages, so long as there is no immediate danger, until the patient is exhausted, and occasionally until the patient slips through their fingers. Several cases have come within my knowledge, where death has been caused by repeated bleedings from the arteries of the extremities, while the physician has looked complacently on, hoping when there was no ground for hope. And a large number of cases I have seen, where life has been left in jeopardy and the constitution injured by extreme losses of blood, before efficient means have been resorted to.

There may have been something peculiar in the constitution of the case of Mr. Skey, and perhaps something wrong in the blood, but a similar condition may be brought about by great loss of blood in very good constitutions. The fibrin and corpuscles being exhausted, very little if any reparative process goes on, and, in this condition, that unaccountable arterial excitement takes place which is called hemorrhagic action under which ligated arteries give way, that in a calm state of the circulation would be safe. In such cases every means should be adopted to enrich the blood; the arterial system should be controlled by opiates, and the arteries properly supported by compress and bandage. It requires some experience to apply compression properly, and a correct knowledge of the principles upon which a cure is effected by it. In a fresh wound the compression should be firm, immediately over the mouth of the divided vessel, and continued until the coagulum is formed in the artery, which ordinarily takes place in a few hours. After this, the parts should be relieved of the strong pressure; for

more moderate pressure than was required to arrest the flow of blood will serve to sustain the clot in the vessel against the internal impulse from the heart. If strong pressure is continued, lymph cannot be deposited around the mouth of the vessel, and secondary hemorrhage will be more likely to occur than if it had been left to nature, without any pressure.

To illustrate my meaning, I will refer to wounds of the vessels of the scalp, where it is so easy and convenient to make strong pressure, and where secondary hemorrhage occurs more frequently than in arteries of similar size in any other part. I am sure that it is often caused by too much pressure; for if the artery is divided again in a healthy place, and pressure made with the finger until bleeding is arrested, and then left to nature, bleeding will rarely recur.

But to return to the palmer arch, I repeat, that if a wound of this vessel is dressed on the principle mentioned above, secondary bleeding will rarely occur, if ever, if the reparative process in the wound is healthy; but if it should, from any cause, we must depend upon granulations to block up the mouths of the divided vessel, and consequently the granulating process in the wound must not be interrupted by too much pressure. This I have found to be the general fault in dressing. Lint pressed tightly down upon the mouth of the bleeding vessel, or into the point where the blood is seen to issue, or, at most, not pressed so firmly as to interrupt the granulating process, will serve to support the clot in its mouth while the healing process will go on. Even if bleeding should again recur, and everything is going on healthily, it will be proper to trust to compression, if the patient is in condition to bear a slight loss of blood without serious injury; but the same care should be taken in making pressure. Under the idea that the bleeding was for the want of pressure, it is increased at each recurrence, and all chance of a cure destroyed. It is in this state that they have been generally admitted into the hospital: the wound unhealthy and sloughy from too much pressure, when the ligature to the radial and ulnar is the true remedy, as it is in all cases where secondary bleeding takes place, and the wound is unhealthy. And it is against delay in such cases that I would remonstrate. The ligature is the surest in all cases; but where the patient and the wound is healthy, we may risk bleeding that will not ultimately injure the patient, for the prospect of avoiding an operation; but when the wound is unhealthy there is no prospect of a cure by compression, and much bleeding increases the danger, and renders the operation less certain, or at least increases the danger of a secondary bleeding when the ligatures

come away, as in the case of Mr. Skey, though I think that if he had given proper support to the arteries at the point where they were tied down, he would have succeeded with the first operation.

My object in this article is not to diminish the confidence of the profession in compression in the treatment of wounded arteries in appropriate cases, but to urge the absolute importance of the ligature in certain cases. There is a time and place for both remedies, and whoever is exclusive in the use of either, is greatly at fault. The same principles hold good with regard to the management of wounds of all other arteries. There is nothing peculiar in the palmer artery, except that the radial and ulnar arteries run into each other, but when both are tied, there is as little danger from anastomosis, as from any other arteries ; indeed in the extraordinary cure of Mr. Skey, there was no recurrence of the bleeding from the original wound, and the assertion of Mr. Arnott that the operation is inefficient, is a bold one, and the assertion that it is unnecessary, is a very injurious one, if there are those who have confidence enough in his judgment to follow it.

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## An Introductory Lecture,

*Delivered to the Medical Class of the University of Louisiana. Session of 1855-'56.*

*By GUSTAVUS A. NOTT, M. D., Prof. of Materia Medica and Therapeutics.*

*Gentlemen :* — It is but right in opening a new course to give some general outline of the objects to be pursued. But time and the opportunity will not permit a satisfactory detail of them. On such an occasion as this, we must look upon the attempt as but the partial preface to a book. To complete that preface, requires many such preliminary discourses. We are to presume you from the requirements of this institution instructed on divers points that you are probably ignorant of. We are, also, to presume you ignorant of many subjects on which you may be well informed. Such necessarily must be the material of all classes and teachers under similar circumstances. The difficulty then is in my position, to know how much or how little to say, for fear of shocking the acquirements of the one, or leaving unenlightened the more modest pretensions of the other. You will bear with me then, whilst I welcome you to these halls, with the sincere wish that you may be profited by my instructions, and that my facilities of imparting knowledge should only equal my earnest desires.

Gentlemen — you are here for the purpose of acquiring an honorable profession. You are here with that youth, health and energy, that lends you the assurance of compassing your object. You are here to pursue various branches seemingly incongruous, but when properly understood, easily combined in our harmonious whole. Remember, the Architect has been the carpenter, the engineer, the apprentice; the man of letters whom you look up to with just admiration, has in earlier days cried over the first letters of his alphabet. The mathematician, who with his abstruse calculations, astounds the world, painfully conned over addition and subtraction, and stood back aghast from the mazes of the multiplication table. The child becomes the man, the man the philosopher, and that philosopher, the child again, not from the common adage and the frail physical tenure of nature which returns to itself, but as from the vastness of his knowledge, he sees how much is yet to learn, and turns with a sigh, still to “pick up pebbles by the sea shore.” Reflect, gentlemen, that the brightest ornaments of our profession, have started on the same path that you have chosen to pursue. Not rushing over obstacles until exhausted and falling by the way side, but slowly and surely, and patiently overcoming difficulties, removing impediments, keeping correct observations of their course, and finally arriving by the shortest road, at the hoped for eminence. The branch we are preparing to follow together, naturally antedates all others, as means of relief from human ills must have been sedulously sought from the first opening of Pandora’s box. Various causes have retarded its progress; resulting either from the inherrent difficulties of the subject, its small adaptability to the rules of inductive reasoning, or from the weakness, credulity, ignorance, superstition, or knavery of mankind. Dread of suffering, faith in bold imposture, and over zealous enthusiasm, still form stumbling blocks to its pursuit. But, divested of these embarrassments, difficulties enough present themselves in man’s changing organization, to lead us to ponder well upon the threshold, to step cautiously, and observantly as we advance, and to inquire humbly and earnestly into that knowledge of nature’s mysteries, which we cannot with the strong hand wrest from her. There are but few subjects, which as a general rule, have been so injudiciously studied as the actions of medicines, and their dependence upon controlling influences around them; as there are none that gives a fairer field for true observation, or that offers a severer test of the discriminating faculties of the human mind.

In their administration, stereotyped routine but too often occupies the place of mature reflection, as custom endorses error. If the human system



were governed by those fixed and immutable laws which rule inanimate matter, our difficulties would disappear as we rose step by step, through known and permanent gradations, and the theory of medicine would thus be simplified to the much to be desired certainty of a mathematical problem. This desideratum has been long sought. The hope has even been fervently entertained, as men of science wrap'd in the mantle of their own thoughts, lost in admiration of the discoveries human genius has accomplished, and entertaining the fond belief in its illimitable extrusion, have walked forward to its attainment, with that single hearted earnestness with which they awaited the era of the philosopher's stone.

It is true, this human frame obeys to some extent, the physical laws of matter, but there is within a ruling principle which the chemist cannot reach, the mechanician control, or the philosopher fathom. Hence, theory after theory has arisen, and perished with those who gave them birth, empiricism, at times, has triumphed over reason, and scepticism obtruded and swaged the intellects of men. Still, though it be not in the designs of nature, that finite minds should unravel those latent secrets, which in her wise laws, are perhaps better concealed, she reveals by intimation many phenomena, which attentively considered, lead to beneficial results in the amelioration of our condition.

I propose to-day, gentlemen, to touch briefly upon a few points, to show the nature of the embarrassments we shall have to contend against during this course; the caution to be exercised in fear of inculcating erroneous opinions, and incite your minds to an earnest consideration of the value of the study you are now preparing to investigate.

We have in the *Materia Medica* many articles, the action of which is supposed to be conducive to human health. The history of these, their localities, growth, or formation, preparation, use and application, have to be studied and investigated. There are others again, which in the main, may be reckoned destructive of life, yet, which under suitable conditions tend to prolong its tenure, which deserve no less attention. The one are medicines, the other poison; and yet, these may be by incautions use, convertible terms, the line of division, as yet, having never been distinctly drawn. The study of these, for convenience sake, are formed into the divisions of *materia medica*, and toxicology. Their preparation, compounding, combining, etc., gives rise to pharmacy. These all form part of my duties during this present course. Occupation enough to allow us but little time to tarry by the way. But this is not all. The most important object still

remains — their practical application. You may describe accurately a carpenter's tool, or an agricultural implement; you may even know well for what purpose they are designed, and still be unable to use them. A new invention in machinery may be eminently useful, or sorely injurious, accordingly as it falls into competent or incompetent hands. The smith must wield, the machinist contrive, the mechanic execute, the master-operator put in motion and develope its usefulness. So with us. The herbalist and mineralogist may collect and classify, the pharmacist prepare, the apothecary dispense; but the physician must apply and become the therapist. It is by keeping this object continually in view, that we bestow an interest on the rudest drug our cabinet contains. Based on physiology and pathology, anatomy being the starting point, we rise to a higher view, and betake ourselves to the study of modifying influences. There is scarcely a condition around us or within us, even the air we breathe, the water we drink, the food by which we are supported, which may exert an influence bearing on the medicines we are provided with, or which may not become remedies in themselves. Most of these will furnish topics for future consideration.

Now, whilst we may not undervalue the long list of articles already known with which the *Materia Medica* is enriched, whilst we regard with satisfaction the additions which are continuously being made to its store, whilst we feel grateful for the untiring energy and labor with which their qualities and effects have been investigated; we can but regret that in many instances blind routine and devotion to authority have made us disregard those collateral circumstances upon which their true utility depends. There are but few who have pursued the practice of medicine for any length of time, and that think at all, who are not convinced that the diversity of opinion, with regard to the action of most of our prominent remedial agents, is dependant upon their hackneyed or unreflective administration. Would sane or reflecting and candid mind deny the incalculable suffering that has been inflicted upon the human system by the various preparations of mercury? Will any one really sincere, and not the unfortunate victim of prejudice, withhold his assent to their value in relieving or discussing pathological derangements of the most painful and harassing character, when judiciously administered in dose, time, and condition? Can we doubt that by overlooking the causes by which symptoms are produced, and disregarding the various surrounding influences, the most opposite effects may be brought about by one and the same remedy, or even by the quantity, combination, or manner in which it is exhibited? Blood letting is unquestion-

ably one of our most available sedative agents, calming exalted action and leading to quietude and repose. Tending directly to debility and loss of power. But in numerous instances it is only relatively so. Within certain limits directly contrary effects may result from its use. In local congestions where the system is depressed, the pulse laboring and irregular, the powers of life, "struggling, faint and low," even as the vital current flows from its impeded bed, the circulation becomes equalized, the pulse assumes its normal volume, and we see strength and vigor induced by an absolutely depressing agent. It were useless to enlarge upon the reverse of this picture. In the words of a distinguished author, Pavis, "From the too common error of mistaking action for power, and of regarding that which is in fact only borrowed for an actual increase of capital, the practitioner has often concluded, that debility and stimulants constantly stand towards each other in the relations of disease and remedy." The time is passing, gentlemen—would it were passed—for the belief that certain remedies, will cure certain diseases, irrespective of pathological conditions, well considered symptoms, and modifying influences, be these of age, sex, constitution, temperament, hygienic observances, or climatic influences. And though classifications be surely necessary, we should, whilst pursuing them ever, remember that stimulants may depress, tonics so called enfeeble, debilitants restore health, narcotics induce wakefulness, cathartics constipation, and in fact any or all of our classified agents, when thoughtlessly used, liable to produce any effects but those which are desired. And though progress is desirable in medicine, as in all other avocations, that progress should be made in the proper direction.

Willingly as we should welcome all new contributions to the already extended list of the articles of the *Materia Medica*; gladly as we would hail any innovation likely to relieve or lessen the burthen of the least of our many infirmities, it would be well to pause and reflect, whether, while in pursuing the phantom qualities of the new, we may not be overleaping and neglecting the well tried virtues of the old. Our journals teem with new remedies thrust forward by hasty adoption, the most of which, like their conjurers, are destined after mature trial, to fill their allotted space in the great lumber house of medicine. Many with unquestionable virtues, like individuals, will suffer the penalty of being overpraised, and be cast aside for not fulfilling unreasonable expectations. It is but in the revolution of centuries that we find in medicine, agents standing forth in such bold isolation, that time will afford us no substitute for them. Such is the era of mercury, opium and the cinchona tree.

But in sifting the incongruous mass with which our Pharmacopiæ are embarrassed, we have ever brought to view some wholesome seed which will germinate to usefulness. Abuses we cannot prevent, either in society or in medicine, caused by ignorance or design. But we may do much by establishing such landmarks as will forbid their being run into. To show with what facility a right may be tortured into a wrong, we may take, in the way of familiar illustration, any of those curative agents which have risen into prominence during our own brief day. But a few years have elapsed since cod liver oil attracted, in a marked degree, the attention of the medical world, for its palliative or curative effects, in certain affections of the lungs, and other diseases consequent on, or accompanied with derangements of the nutritive functions. Since its introduction, it has met with such signal favor as to have become of almost universal employment. It is not my purpose now to inquire, beyond the narrow limits to which I have restricted myself on the present occasion, how far this favor is deserved.

Without discussing the question, whether the beneficial effects known to occur under its use, may not be mainly attributable to other adjuvants associated with it. How much selected and nutritious diet, fresh air and exercise, counter-irritation, and quietude of mind from assurance of benefit, may do. But according to the fullest extent the virtues claimed for it, it will not be amiss for us in this inquiry, to throw a passing glance on the empirical recklessness with which it is used and abused, not alone by the medical practitioner, but by the great self constituted mediciner, the public at large. There are few remedies even with more eminent qualities that would stand the test. Should an individual present the appearances of failing health; should there be emaciation of body, or more unfortunately for him, should a troublesome cough appear, these external symptoms are usually enough to call upon the never failing banks of New Foundland for relief, either by the routine practitioner himself, or some kindly hearted, peripatetic, friendly nuisance of a community, who has known within the circle of his own observation vast numbers of cases similarly affected, cured by this potent remedy. Is it not lamentably true, that the causes of these symptoms, and the general condition of the patient, is but too seldom inquired into? Debility, emaciation and cough constitute phthisis, and cod liver oil is its remedy. By such fallacious reasoning do many of our medicines acquire undue importance or undeserved censure, through unsound diagnosis, even by the means that cancer curers attain wealth. Let any one who has ever entered the Charity Hospital with an observant eye, (it



holds good without the walls of that noble institution), and say that he has not seen delicate stomachs revolt at the viscid dose; that in its pertinacious use in the lost hope of establishing tolerance, he has not seen fatal irritability of the gastro-intestinal tube increased or brought about in despite of that loathing which nature throws out to us as a hint to withdraw our hand. I have no desire to multiply examples. The agent that I have casually taken stands not alone. Bismuth, glycerine, manganese, and what not, stand in the same category. I shall recur again to this subject before I conclude.

Let us turn for the present in investigating modifying causes to some of those diseases which are common around us, and to prevent all misconception, let us boldly face, the great scourges of our Southern cities, yellow fever. It may be that I will advance propositions that will meet with no friendly response, at least, with the great mass which constitutes the community. Be it so. They will not be less true on this account. But I will appeal to any physician of enlightened experience for their correctness. With the history, progress, causes, pathology, or special treatment of yellow fever, I have nothing to say. These considerations fall under the province of others better fitted to elaborate them. It concerns me only so far as they have a bearing in modifying remedial agents, be they material or immaterial, (for in both of these classes we have many.) It has been said, that the per centage of deaths from yellow fever, has been about the same at all times, in all localities, and under all methods of treatment. This *may be* true, (but which for one I do not believe,) in summing up the mortality of various localities; because the same condition exist in almost all, as far as quackery, neglect, ignorance and interference are concerned, together with the peculiar nature of the disease.

But it certainly is far from true to say, that as many would recover under one mode of treatment, nursing and general management, as under another.

And now, I maintain unequivocally that yellow fever is not the fatal disease it has been represented to be, where reasonable prudence is observed, and that medicines under certain reservations to be mentioned, exercise as beneficial an influence in this, as in most other diseases. It is bad enough, however, under any circumstances, but let us not unduly exaggerate evils which are sufficient as they stand. Regard the population that dwell upon the borders of our Southern and Western streams, and observe with what unconcealed dread they look upon this supposed pest-house of calamity, New Orleans. Why should they not turn their eyes within and realize the

dangers of their congestive fevers, (the fatal country fever of the Carolinas,) that kill with almost the certainty and celerity of the rifle ball, when momentarily neglected; or their mortal dysenteries, and pneumonias devastating whole sections at one fell swoop? And where the individual escapes, no guarantee of future exemption? Yellow fever may strike hard; but it is a generous foe, and strikes but once. But let us examine into the causes of the great mortality occurring from this disease, as they relate to our subject, independently of any special or peculiar medicinal agents. Where, and how, and under what circumstances these deaths take place.

I have already stated that I neither wished to discuss the pathology of this disease, nor any particular mode or modes of treatment. Divers opinions are entertained with regard to both these points. But leaving these alone as irrelevant, there remains some interesting facts upon which we all agree. The nervous system has received a shock. I inquire not what lesion has occurred. I ask not whether this nervousness as we term it, be primary or secondary. I simply state the acknowledged fact, that it does exist, and has to be carefully watched as the disease advances to its termination. We must bear in mind as we proceed, the peculiar susceptibility of patients suffering under nervous disorder to mental influences, the impressible nature of these diseases to surrounding excitements, and the liability to sudden exhaustion in the one under discussion.

No physician of any observation will deny their importance, in hysteria, hypochondria, tetanus, and kindred diseases, and the little utility of medicine, unless these conditions are carefully observed.

Probably, the general course and treatment of yellow fever is understood as well in this city, as in any other in the world; and why should it not be. The physicians, if they do not look for, are not astonished at its annual invasion. The real citizen regards its advent but as a call upon those kinder feelings, which during the business season have been rusting under the shadow of the potent dollar. He fears but for his friend or the friendless, and for these, in a great measure, he feels assured if proper precaution be observed, they are but taking the initiatory step of summer health and permanent usefulness. Because, he can but well know and deplore that absenteeism of person, with its consequent absenteeism of capital, is the bone of that city where he has staked forever his hopes and fortune. Keeping in view then what has been said, let us now look around us for those causes, which during its prevalence, render our bills of mortality undoubt-

edly great. Nearly nine thousand having perished alone in 1853, with a greatly diminished population.

Let us look into the mansions of the wealthy, as well as into the hovels of the indigent. Let us also glance at the middle state. It may be as in the common events of life, we will find the two extremes meeting. The highest and the lowest equally touching their hats, in observing the civilities of the day.

We shall find, perhaps, that it is not always where wealth and fashion dwell, that we can hope to witness the most beneficial effects from our remedial agents. The disturbing cause is there whether we have too much or too little attention paid, whether we have too much or too little anxiety evinced. The pressing solicitude of friends, who in mistaken kindness, besiege the sick room, over zealous proffers of attention, even the mottled consultations which wealth calls around, but too often overbalances any aid the *Materia Medica* might bestow. And in many of those cases which have been nursed and fondled out of life; had they have been in humbler circumstances, with one physician, a sensible nurse, and but few friends, the tale told might have been far different. Whilst I speak in these terms, I do not believe in the common cant of the day; that the professional nurses of New Orleans, may safely take into their own hands the treatment of this most treacherous disease. On the contrary, it is to be feared that in many instances, the initiated nurse does more harm than good, not only in volunteering in opinion, but in action, and that the patient is frequently worried from a good condition to a worse one, professedly for his own benefit; but really to show how much the nurse knows. But to return. We have had within the limits of this city, abundant opportunities of witnessing in a very forcible manner, the effects of disturbing causes. It is within the knowledge of many of the physicians of this place, that during the memorable epidemic of 1853, whilst the citizens and authorities were devising any and every means to stay the pestilence; one of the plans adopted and put into execution, was the firing of cannon at the street corners; and that during this cannonade, patients who under the calming influence of remedial agents, were considered in a fair way to recovery, instantly showed symptoms of fatal nervous derangement, from which they could not be relieved. And so with many minor imprudences, either on the part of the patient, or that of his friends. It is so difficult to impress upon persons unaccustomed to this disease, its treacherous nature, that because they feel well, they are not so; that the very slightest physical exer-

tion, or mental excitement, may turn the balance against them. That uncounted numbers have paid the heavy forfeit of their incredulity.

Let us quit these scenes of comparative comfort and look into the dwellings of the destitute. If medicine can be shocked from its purpose where wealth, and power, and troops of friends exist, how can we wonder at the great fatality amongst the more lowly and friendless? Here scenes present themselves too forbidding for the imagination to grasp, where penury forbids the necessities of life, and even the free air of heaven itself is denied. I have seen within the compass of a room twelve feet square, as many persons stricken with yellow fever. I have seen here the dying within the embrace of the dead. I have seen the dead hastily removed to make room for the quick, soon to follow in their path. I have seen the stiffened corpse, stretched in its long, last sleep, and no one to tell how or at what time the vital spark had fled from its frail tenement. Amidst such scenes of distress, and despondency, and with a disease so peculiarly influenced by outward circumstances, we cease to inquire what especial mode of treatment would give the best results. Again in our Charity Hospital, which opens wide its portals at the slightest appeal from suffering humanity, and from the returns of which the community, not only here, but elsewhere, are wont to form their opinions of the virulence of the disease, at least forty per cent. of the afflicted are removed thither by unfavorable means of transportation at the very crisis of the disease, when quietude and repose are the essentials of success. Witness the baneful effects of depressing influences in those localities unaccustomed to the ravages of this disease. In some of our own river towns unfamiliar with its nature, where panic usurps the place not only of reason, but the ties of affection, fifty per cent. of the whole population have fallen victims to it.\* Witness the harrowing details of Norfolk and Portsmouth, and reflect what would be its effects in such a populous city as New York, should it prevail after a lapse of so many years. The mortality would depend not so much upon the particular mode of treatment pursued, although due consideration for this is admitted, but from the terror of, to them, a new and over exaggerated disease, prevailing in a population containing too few of the acclimated to give the least confidence or hope.

It may be asked can nothing be done to remove these conditions? I believe much may be done by proper sanitary regulations. But these questions extend beyond my province here. And now, gentlemen, I do not

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\* Lake Providence and other localities.



wish to be misunderstood as underrating in any degree the dangers to be apprehended from this dreaded scourge. But I return to my first proposition. That by avoiding undue excitement, by quieting fears, by keeping out of view, and removing from the mind, the harrowing evidences of disease around, by employing nurses that will do what they are told to do, and nothing *more*, our remedial agents will act with their accustomed promptitude, and yellow fever be deprived of many of its terrors. These points would be more fully appreciated were it possible to classify, how many have lost their lives by over-treatment, how many through the anxiety of friends, how many through neglect, how many through injudicious and uncalled for interference, and how many through the imprudence of the convalescent patients themselves. Could these facts be established or even approximated, we should hear fewer discussions with regard to this or that particular list of medicines; absolute routine would cease amongst educated men, and common sense assert her reign once more.

Tetanus again is one of those diseases, which in this relation we can study with much profit. Not merely in respect to the tolerance of the system of almost incredible doses of narcotic poisons, but more especially in reference to those exterior influences that change, or pervert the usual known or accepted actions of medicines. There is, perhaps, no disease in the whole catalogue more distressing to the friends of the patient than this, and none in which repressed anxiety produces more baneful effects. Those seeming evidences of suffering are apparent to the eye at once. The distortion of feature, and dreadful wrenching of frame, appear to call promptly for sympathy and interference. It seems a cruelty on the part of the physician to deny access to them; and unfortunately, all have not the firmness to do their duty; and still more unfortunately for human nature if they did so, they would probably be displaced by others more complying. And from what apparently slight causes—a draught of air—the abrupt entrance into a room—the invasion of light even, are we doomed to witness the total failure of our most reliable agents. But I trust I have said enough in throwing out these hurried hints, to cause you to reflect that medicines in their application to diseased conditions, must be considered as mere relative agents. That without weighing all collateral points thoroughly in administering them, they must disappoint your best expectations, and may, as has been done, induce a scepticism as to the virtues of any. In the long list of the articles of the *Materia Medica*, there are many that possess an interest in themselves, in their history, combinations and effects, as *Anatomy* possesses an interest even in its special detail. But when the view is enlarged,

when we see the application for which they have been studiously acquired, the pleasure increases as the circle of observation extends, and we feel those calls upon the reasoning faculties, which, contrary to vulgar opinion, is not exceeded by any avocation in life. It is singular enough, I may observe in this connection, that in the common transactions of every day occurrence, scarcely an individual is willing to risk his rights of property by drawing up for himself, or neighbor, the simplest law form, where every thing may be done, even by printed rule; yet feels himself competent to advise or administer in those more subtle pathological derangements on the management of which, depends all that renders property desirable. In those cases in which we see the experienced and well informed physician pause to reflect, where the powers of his mind are strained to the uttermost, where he is calling to his aid (as much often by inactivity, as action), all the resources of his art: we see these Gordian difficulties cut by boastful ignorance, "and fools rush in where angels fear to tread." It is but by watching with an attentive eye the group of phenomena presented to you or him, by taking comprehensive grasp of the collateral circumstances surrounding them; by carefully considering the relation of remedies to lesions and symptoms, that you can hope to wrestle successfully with disease, call that disease by what name you will. Wide is the area of the circle sketched. Genius and observation have extended it. Patient investigation and unselfish labor is adding to it, and science has hallowed it.

Let us turn to one other fruitful source, if not of error, at least of disappointment in practice, and I will have done. I allude to the floods of adulterated medicines with which, either through design or ignorance, the country is inundated. It is to be deeply regretted, that where the pecuniary interests of men come in conflict with human ease, comfort or health, the "balance is rarely, right adjusted." This subject has not received the attention its importance demands. We are told that with regard to one of the commercial luxuries for which we are taxed, that there is more Champagne wine consumed in the United States alone, than the whole Champagne country will produce. Of course it is either manufactured here or in foreign countries, and is not unfrequently flavored with the most deleterious substances. We know that the malt liquors so abundantly used here and elsewhere, often owe their peculiar bitterness to one of the most deadly poisons that we have. (*Cocculus Indicus*).

We know that many of the necessary articles of aliment in common daily use, are subject to being fraudulently prepared. But we give but little heed to an occurrence equally common, and certainly not less import-

ant, the introduction and manufacture of spurious or adulterated drugs. Physicians are too much in the habit of writing their prescriptions, giving directions for their administration, and with this, dismissing the subject from their minds. Should they on a subsequent visit find that their expectations have not been realized, they are equally apt to attribute the failure to the virulence of the disease, without bestowing a thought on the probably true collateral cause, the impurity of the drug. Some years since the attention of Congress was directed to this subject, and inspectors were appointed in the different sea-port towns, to prevent the *importation* of spurious or impure articles. Unfortunately for the scheme, like numbers of our federal appointments, many of these officials have been chosen, not so much from their competency to discharge this duty, as from political motives. I have been enabled to obtain but one report; that of Dr. Bailey, the very efficient inspector of New York. The developments that have been made by him are startling in the extreme. But supposing this inspection to be ever so efficiently performed, it is very far from reaching or abolishing the evil. For we are informed that a very large drug establishment in Europe, (Switzerland,) which had been extensively engaged in the preparation of adulterated articles for the markets of the United States, immediately after the law had passed, interdicting the *importation* of such articles, quietly removed to the United States, and continue their nefarious traffic unmolested. These subjects will receive due consideration in their proper place. But I have thought it necessary to allude to them now, that you may understand my meaning in any subsequent remarks.

I have already alluded to several articles of the *Materia Medica* in endeavoring to elucidate our point. As they may suit my purpose just as well as any others, I recur to them again, in attempting to explain another. Many of these articles, I have stated, fail to produce expected results, when perfectly pure themselves, through injudicious, thoughtless, or ignorant administration. But we will concede to the physician all the endowments he should possess: that he has knowledge, tact, judgment, observation, and that he discriminates well. He has made out his case, and has chosen his medicinal agent. He has perhaps given his patient hopes of relief. Now, suppose cod liver oil to be that agent, and it fails in producing this effect. The patient may lose confidence in his medical attendant, the physician in his remedy; and yet both be equally unjust. Now it is a well ascertained fact, that our markets abound with spurious imitations of this oil, which are probably not only worthless, but which may contain ingredients abso-



lutely hurtful. Or, perhaps when obtained from the proper sources, it becomes possessed of deleterious qualities from careless or unskilful manipulation. Should either be the case, we can but be disappointed in the results of its use. Again, we are instructed, believe and even know, that in given conditions of the system, opium will afford exemption from pain and induce sleep; on the procurement of which life frequently depends. In such cases we prescribe it with almost the positive assurance of effecting our object. Now, suppose tincture of opium (common laudanum) to be the preparation we select. We are to presume that one drachm of this tincture is equivalent to about four grains of opium; that morphia is the principal active ingredient of this drug; and we recommend it from these data. Now, suppose over again, that this active principle, morphia, has been already extracted from the opium previously to the formation of the tincture. Although our diagnosis may have been correct, our judgment sound, our prescription apposite to the case; our reasonable expectations must result in failure through circumstances over which we have no control.

Mercury, too, is a substance that I have mentioned to you in an other relation. Let us examine for a moment some of those preparations of it which are most commonly known. Calomel is one (proto-chloride) which we frequently use under circumstances in which we can find nothing to supply its place. We expect it to lie quietly on the stomach and to operate mildly on the system. We prescribe it with these intentions. To our surprise, we find it producing great gastric irritation, operating so harshly and unkindly as to induce us to suspect something unusual. We examine, and find (charitably) that from careless preparation or preservation, it contains a large proportion of the poisonous compound,—corrosive sublimate, (Deuto-chloride). Blue mass is another familiar example, in almost daily use, of the compounds of that most interesting metal, through which we wish to obtain its peculiar attractive effects. The object we have in view, say is, to promote absorption, to discuss effusions or restore secretions. We advise it accordingly. Not expecting immediate results, we patiently wait until its slow, but specific effects may evidence themselves. But we wait in vain. The drug contains but one-tenth of the due proportion, or perhaps not one particle of mercury. You have been using an adulterated and useless article. These are not imaginary evils conjured up for a purpose—they unhappily have but too real an existence. For it has been shown in our various drug stores (I do not allude particularly to New Orleans, for we are perhaps more secure from these impostures than most large cities, although we have a wide range of margin), that there is dispensed opium



without morphia, chinchona barks deprived of their quinine, blue mass without mercury, powders of various kinds made from fictitious barks and roots, tinctures innumerable destitute of active qualities, and even the alkaloids themselves weakened by attenuation with amygdaline, starch, manite, lime and other cheaper inert or deleterious substances. It is true that by chemical processes more or less complex, we can detect and in some degree provide against these abuses. But it is physically impossible that the general practitioner can avail himself of this resource in the numberless articles he is daily called upon to prescribe.

But I have said enough to give an intimation of my meaning, and it is unnecessary to enlarge upon the topic. I leave the subject, but, not for the reasons I have seen advocated elsewhere. For I am surprised to find that in the last meeting of the "British Association for the Advancement of Science," on Dr. Hassal's reading an elaborate report on chemical adulterations, and pointing out the substances most commonly subject to them, much excitement was caused, and the paper objected to as being impolitic, as such truths should not be discussed, lest too much alarm might be created in the public mind. For myself, I have no fears that such alarm could result in any thing but good.

Now, gentlemen, you are about to enter upon the study of *Materia Medica*. I have endeavored to point out to you some few of those conditions and circumstances which may exercise a controlling or modifying influence on the actions of medicines. There are yet many others, the examination of which will occupy our attention for some time to come. For I sincerely believe that should these principles be disregarded, it would have been far better that all those remedial agents so laboriously brought to light, should have slumbered forever in their original obscurity. Divest your minds at once of the idea, if the impression exists, that *Materia Medica* is the dull routine that calls for no other exertion on the faculties of the mind than that of memory. If you have reflective powers, rest assured they will be taxed to the greatest extent. But I am most fortunate, at least in one respect, in having an unpopular chair, that I am necessarily assisted by all my colleagues. The professor of Surgery, the true surgeon, does not move without elucidating important points in my branch. The professor of Practice is compelled to assist you in acquiring a knowledge of the action of drugs. The Physiologist and Pathologist must explain points by them. The Chemist refers to them in relation to their purity. The Obstetrician dilates upon them. And even the Anatomist drags them in to make more plain a known fact.

Thus, has it happened to me, through these aids and the respectful attention of the class in years gone by, to have the pleasure of seeing the candidates for graduation, at least respectably prepared on this most difficult, but most important branch of medicine. Gentlemen, I expect no less of you.

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## Yellow Fever in Thibodaux, La., in the Year 1854.

*By M. A. McLEOD, of Thibodaux.*

Previous to the appearance of the yellow fever here this year, we had very unsettled weather—rain almost every day, yet, there was nothing in or about the town that would indicate sickness of any kind, and up to that time during the summer, the place had been remarkably healthy.

The first case occurred in September. On the 12th day of that month, I was informed that there was a case of yellow fever in town; the sick man was an Italian, who had recently returned from New Orleans; yet, the attending physician did not pronounce the case yellow fever, (perhaps not wishing to create an alarm.) But my informant was one of the nurses, and non-professional, but he had seen and nursed a number of cases of yellow fever the previous year, and he did not hesitate to pronounce this a well developed case of that disease. The Italian recovered. On the evening of the 20th of September, some of our unacclimated citizens became alarmed; it was reported that a tailor on the opposite square from where the Italian was sick, was dying of yellow fever, and I was asked to go, and if possible, ascertain the correctness of the report. I called at the house next morning, but the man had died during the night. I met the attending physician, (who was the same that had treated the Italian,) but he gave me very little satisfaction in regard to the case, and stated that the patient had thrown up black matter, but he rather looked upon his disease as cholera.

~~and~~ September 22d, a woman, near the above, and on the same street, and opposite to where the Italian was sick, died this day of yellow fever, pronounced such by her attending physician, Dr. J. W. Deadrick. A German, gardner, who resided four miles above the town, was in the place, and assisted in putting the corpse in the coffin, and attended the fune-

ral. He went home, and a few days after he was taken with fever and was removed back to town, and on the night of October the 4th, he died with black vomit. This was likewise the patient of Dr. J. W. Deadrick, and it was the first case of black vomit I saw this year. About this time, the fever appeared to have spread pretty much throughout the town, and all our physicians reported a greater or less number of cases, which alarmed the unacclimated, many of whom fled to the country, and remained there secure until we had frost.

Now, I have no doubt but the first case of yellow fever here this year, was the Italian's, and he brought the disease from New Orleans, and from him it spread throughout this place. We could not trace it distinctly from one person to another, yet, there are some few cases where we could trace it distinctly, such as the following :

An Episcopal minister, (Mr. Trader,) who came to reside here last spring, and was unacclimated, visited a young man who lay sick with fever, and a few days after, he went about a mile in the country, (at Judge Guion's,) to dine, and after dinner it rained, and he was prevailed on to spend the night ; some time in the course of which, he was taken with yellow fever, and remained there until he was convalescent. A few days after this, the family where this man was staying, were taken with the fever, and one, a very interesting daughter died. A son-in-law of Judge Guion, Dr. Young, who resided in Houma, sixteen miles from this place where there was no yellow fever, came up with his lady to attend on the sick, and while there, they were both taken with the fever, and the doctor died.

There are other instances here where the disease appeared to spread by contagion, and in fact, it was far more traceable from person to person, than an epidemic of small pox has been, which we have but recently passed through here.

A man came from New Orleans here with small pox, and so soon as it was ascertained that he had the disease, he was removed without the limits of the town, where he died, and his clothes were there burned. Yet, in ten or fifteen days after this, the small pox, broke out in all quarters of the town, the cases within a few days of each other, and in the most cases it was in persons who had not been near the sick man, and I look upon it just as easy to account for the spread of one, as the other. I do not pretend to say that the contagion of yellow fever, is as virulent as that of small pox under all circumstances, yet, it may be so in tropical climates where it is endemic. I look upon yellow fever as an imported disease, and whenever this or any other climate assimilates in temperature and other

things, that where yellow fever is indigenous, let the contagion once be started, and it will spread from person to person, until we have a change in temperature to check it.

The fever continued to attack the unacclimated until we had a killing frost, which occurred on the 15th of October, and even after this we had a good many cases, but it was among persons who had been exposed to the disease previous to the frost.

I had one recovered after black vomit in my practice. Now, in regard to the treatment, so much has been said, that I deem it unnecessary to speak of that in detail. But I look upon it, that remedies that might be found efficacious in one locality, might not answer so well in another, as the yellow fever, I think, is apt to partake of the nature of the prevailing disease of the particular localities where it spreads, and no doubt requires to be treated according. And this will account in a measure for the different successful treatment in different places, although they may be the opposite of each other.

The treatment I found the most efficacious, was to open the bowels freely at the start. Mustard foot baths and warm teas, until the fever had subsided, and then, stimulants or not according to circumstances. I used very little quinine.

The muriated tinct. of iron I used to some little extent, but not sufficiently to speak of its effects, either pro or con.

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## On the Rudimentary Reproduction of Extremities AFTER THEIR SPONTANEOUS AMPUTATION.

*By Prof. SIMPSON, of Edinburg.*

On the stumps of limbs that have seemingly undergone an early spontaneous amputation in utero, there is often seen a species of anormal structure, which has not as yet, so far as I am aware, been described in any existing work on the subject of monstrosities. I allude to the appearance on the ends of many such stumps of a projecting mass, or nodule, varying in size from a small cutaneous ridge to the bulk of a walnut, and having protruding from its surface one, two or more still smaller fleshy divisions or projections, which are provided at their extreme points with nails.



This variety of anormal structure is by no means rare. Several years ago, while searching for instances of it, I found five or six living examples in Edinburgh and its neighborhood; and I have seen some, and heard of many more, living in different parts of Scotland and England. It is interesting, however, not so much for the frequency with which it is met with, as from the nature of the anormal structure itself, consisting, as I believe it does, of a tendency in the human subject to the reproduction of a lost extremity.

As a general law, the power of repairing and reproducing lost parts decreases as we ascend from the lower to the higher parts of the animal scale. In the lowest and simplest forms of animal life, as in polypes, not only are separate parts or segments rapidly restored, but the separate segments themselves sometimes become developed into whole and perfect individuals. A hydra was cut at different times into various portions by Trembley, and fifty separate individuals of the species were developed from the segments of one. Johnson and Duges have shown that animals with a much higher organization, viz: the planariæ—could in the same way be multiplied by artificial subdivision; and Lyonnet and Bonnet found the same true of the nais. As we ascend upward in the scale of life, all power of self-development in separated parts or segments disappears, but the power of regenerating these lost parts or segments is retained to a greater or less degree by the general body of the animal. When the arms or rays of a star-fish are broken off artificially, or when they are thrown off, as they sometimes are in the lingthorn, or lludia, etc., by a true “spontaneous amputation” on the part of the animal, the lost arms are betimes entirely restored. In crustacea a separated or amputated limb is also rapidly renovated. The head or anterior rings of the earth-worm and other annelida are generally regenerated after their decapitation; and the power of reproduction is still so great in the mollusca, that the snail, according to Schweigger, has sometimes its head and antennæ restored after they are removed by amputation, provided the cephalic ganglion lying above the œsophagus be left uninjured. In the lower divisions of the vertebrata we have the salamander still capable of re-producing an entire leg or tail, or even of forming a new under jaw; and the triton can regenerate, as in Blumenbach’s experiments, a complete and perfect eye. But in the higher and warm-blooded vertebrata this power of repairing and restoring lost compound parts and organs seems totally, or almost totally, wanting. In man, not only are complex individual parts, however small, generally held incapable of restoration, but portions of the higher individual tissues,

even, as mucous membrane, muscle, &c., when cut, removed or destroyed, are not usually regenerated in their entire organization. To this general law, however, there are the following exceptions in the human subject.

1st. When the part removed is primitively of a lower type of organization than that of the general body, restoration sometimes occurs. Thus, in a case of a child born with an additional thumb, or with a thumb double from the first joint, the outer or smaller one was amputated by Mr. White, of Manchester. It grew again, and along with it the nail. Subsequently, Mr. Bromfield, of London, a second time carefully removed this superadded portion of thumb, and turned the ball of it fairly out of the socket. "Notwithstanding this," adds Mr. White, "it grew again, and a fresh nail was formed."\*

2d. In those animals that possess, in the most marked degree, the power of readily regenerating lost compound parts, this power resides especially in the extreme points of the body, as the tail and limbs. In the human subject we sometimes find instances of an appearance of the same power in the extreme parts, as the fingers or toes. I have seen a distinct but imperfect nail grow on the end of the second phalanx of the finger, after the complete amputation of the first phalanx. Similar instances of nails, and consequently of the matrices of these nails, becoming regenerated on the tips of fingers amputated through their first joint, have been recorded by Corvisart, Ansiaux, Blumenbach and others.

3d. When, in the human subject, the removal of a compound part—such as a portion of an extremity—is effected in early foetal life, and consequently at a time when the physiological powers of the young human being are more assimilated to the reparative and other powers of animals of a lower type in the animal scale, the lost part seems capable of at least a partial and rudimentary restoration. In the animal kingdom generally, we find the power of regeneration greater in the inverse ratio of the degree of development or age of the individual. The more perfect hexapod insects never reproduce a lost limb; but in the larvæ of these same insects, limbs and antennæ are restored after their removal. The experiments of Heineken show that the arachnida, in the same way, lose the property of regenerating their legs after they have ceased to change their skin, and have reached their full or adult development. It is only in the young frog that reproduction of a limb occurs; and Spallanzani found that the rapidity with

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\* Regeneration of Animal and Vegetable Substances, p. 16.

which the tail of the tadpole and the limbs of the salamander are regenerated, was always in an inverse ratio to the age of the animal. So while in the human subject after birth we never see any trace of the reproduction of a limb after amputation, we have the contrary, as I believe, evidence of the possibility of their rudimentary regeneration in the appearances sometimes seen on the ends of stumps resulting from spontaneous amputation in early foetal or embryonic life.

In most of the cases in which I have observed this appearance of a rudimentary regeneration of an extremity, the spontaneous amputation had occurred in the upper half of the forearm; and the general resemblance of these cases to each other is very remarkable. Usually the rounded end of the limb has exactly the appearance of a stump after amputation, and is well covered with soft parts. Two points of the skin, or rather of the subcutaneous tissue, are found adherent to the ends of the ulna and radius, and present a depressed or umbilicated form, particularly when the forearm is flexed or moved, and the fissures of the skin seen in converging lines to these two points as centres. Midway, and a little in front of these two points, the rudiment of the regenerated extremity is situated in the form of a raised cutaneous fold or fleshy mass or tubercle, and having on its surface one, two or more smaller projections or nodules, furnished with minute nails. In the instance of a young woman of 18 years of age, four such imperfect fingers were seen, two of them tipped with nails. In this, as in most other cases, the left arm is the seat of the mutilation, but I have seen the right similarly affected.

The stump of the left forearm of a foetus of the seventh month, is preserved in the Obstetric Museum of the University of Edinburgh, having five small rudimentary fingers tipped with minute nails in the usual position on the end of the stump. But the case is principally remarkable for the circumstance, that the cicatrization over the ends of the ulna and radius is not complete. There is an aperture at the end of the radius, through which the end of the bone can be felt when the point of a pin is passed through it. The ulna projects to the cutaneous surface of the stump, and has a small wound or circle of uncovered granulations still around it; or in other words, the cicatrix of the stump is as yet incomplete.

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#### ON INTRA-UTERINE GOITRE, OR BRONCHOCELE.

Various kinds of tumor in the cervical region of the foetus have been found at the time of birth.

1. The cervical portion of the vertebral column is sometimes, though not so often as the loins or back, the seat of spina bifida; and the resulting tumor has been seen to vary in size from the bulk of a nut to that of the infant's head.

2. Meckel, Otto and several other pathologists have described a variety of congenital cystic tumor of the cellular tissue, situated at the posterior part of the neck, and remarkable for a central pillared division, into two lateral and symmetrical lobes, by the ligamentum nuchæ. I have seen it mistaken for a spina bifida.

3. Numerous instances of intra-uterine tumors in the anterior and lateral portions of the cervical region have been recorded of late years by Berndt, Cæsar Hawkins, Beatty, Mutter and others, under the name of cysts, or "congenital hydrocele" of the neck. These tumors, which sometimes grow much after birth, usually consist of one, two or more large serous cysts, capable of being emptied and obliterated by puncture and the injection of iodine; by setons, &c. Sometimes an agglomeration of small cysts enters also into their composition.

4. A kind of large cystic tumor, the mass of which consists of a numerous collection of small cells filled with thickish glairy fluid, is occasionally observed at birth in the upper part of the neck, and projects more or less into the mouth. It seems to be a true ranula, originating and consisting of hypertrophy and enlargement of the salivary glands. I have seen two instances of it where the children died a few days after birth, the puncturing of one or two cells being of no use in diminishing the mass. There is a specimen of it in the obstetric museum of the University, forming a large tumor at birth. This form of apparent cystiform tumor has, I believe, been frequently confounded with the third variety alluded to above.

5. Among the tumors on the neck at birth, I have seen one remarkable instance of a large flattish swelling on the posterior cervical region, covered with skin of the usual color and appearance, and formed of a deep-seated erectile vascular tissue, which in a great measure disappeared under pressure, and enlarged when the child cried or strained. I treated it by various means, none of which produced complete obliteration. Some years afterwards I heard it was cut out by a surgeon, and the resulting hemorrhage was most excessive.

6. Few cases of congenital enlargement of the thyroid gland, or of true intra-uterine goitre, or bronchocele, have hitherto been placed upon record. The following cases, however, will show that goitre constitutes one form of cervi-



cal tumor, which may be occasionally met with at birth. Bronchocele is sometimes hereditary, but very few instances of it have been seen at birth in infants so predisposed. Usually there is no trace of it till some years subsequently. The following is the only exception to this general remark which I have been able to find:—

*Case I.*—In an essay on goitre, published in 1824, M. Ferus refers to a congenital instance of the disease, which had occurred in the practice of M. Godelle, physician to the Hospital of Soissons, and where the mother was affected with the same disease.\* The child only survived for a few hours after birth.†

Lately I met with a marked instance of intra-uterine goitre in my own practice, and had an opportunity of ascertaining its true nature by dissection.

*Case II.*—The mother of the child never suffered from any symptoms of goitre, or lived in any place where the disease was endemic. She has now borne ten children. The first seven of these infants were stillborn. They all died, I believe from reports given to me, of disease of the placenta, and not from any malady or malformation in their own bodies. During her last four pregnancies she has been under my professional care, and has always taken, in the latter periods of utero-gestation, large and continuous doses of chlorate of potass. The four last children were born alive, and have continued to live, with the exception of the last, namely, the one born with goitre. It survived only for about eight hours after birth, and would have died much earlier from asphyxia if a catheter had not been retained in the trachea to obviate the compression of the mass of bronchocele. The child was born two or three weeks before the full term, labor having been induced in consequence of the child's heart beginning to beat with morbid slowness. The goitrous enlargement of the thyroid gland was nearly of the size of a hen's egg. It rendered the labor tedious, by preventing—as the hands or arms placed in the hollows of the neck sometimes do—the proper flexion of the head, and the approach of the chin to the sternum; the presentation in

\* Dictionnaire de Medecine, vol. x., p. 283.

† In the Archives Generales de Medecine, vol. xiii., p. 76, Dr. Casan speaks of a remarkable case of hereditary goitre, where a young infant in the family died of it; but whether it was hereditary or not in this child, is not precisely stated. "A woman aged 23 years married, affected with pulmonary consumption in the second degree, presented to us an example of the obstinate hereditary predisposition of pulmonary phthisis and of goitre; her young infant (*jeune enfant*), her father, and seven brothers of her father had died of the former disease; one of her paternal aunts, who showed no disposition to phthisis, carried a very large goitre; herself (the patient), was affected with goitre, which had considerably diminished since the first symptoms of phthisis had been developed. All her brothers and sisters had been victims to that cruel affection; only one sister, who had goitre commencing, enjoyed good health at that period. One could say in that family, that the two affections were in such relation, that the one appeared reciprocally to supplant the other."

consequence being one of the forehead, and not of the parietal bone. The goitre, or bronchocele, as seen after birth, appeared to fill up entirely the space or hollow between the chin and sternum. On examination after death, it was found to surround almost entirely, and compress, the trachea. All parts of the thyroid gland were equally affected. The goitrous tumor was comparatively smooth on its surface, but had a small, irregular nodule attached anteriorly to its upper border, close to the body of the hyoid bone.

Internally, it presented a firm, glandular structure, and under the microscope, it appeared to consist of the usual thyroid tissues, greatly hypertrophied. The vesicular cavities of the gland seemed not only increased in number, but enlarged in size also, and the septa within them were thickened. They were distended with epithelial contents. The external surface of the brain of the child was surrounded with a large quantity of serum, and the brain itself was considerably below the usual size. The opening of the eyelids was almost small. The thymus gland, supra-renal capsules, etc., were normal in size and structure; and there was no other unusual appearance detected. In his essay on the pathological anatomy of newborn infants,\* Dr. F. Weber describes an example of congenital goitre, similar in several respects to the preceding instance.

*Case III.* — A child was born some weeks before the ninth month, and it survived only a few minutes. The goitrous thyroid gland projected forward in the cervical region, was about half an inch thick, and extended not only laterally, but also backward, and some distance over the upper part of the trachea, though not to such a degree that a union of both lateral lobes had occurred posteriorly. On being cut into, the parenchyma of the bronchocele appeared dark red, and the microscope showed within it a quantity of effused blood-globules, which were not evident to the naked eye. In other respects, the parenchyma of the tumor presented internally the normal structure of the thyroid gland. The thymus gland appeared also larger than usual, and particularly on one side, but without any change of structure. There was a considerable degree of hydrocephalus present with contraction of the extremities.

*Case IV.* — When describing the case No. II to the Medico-Chirurgical Society, immediately after the time of its occurrence in February, 1855,†

\* Beiträge zur Path. Anatomie der Neugeborenen, p. 84.

† See report of Society's proceedings, Ed. Monthly Journal of Medical Science, April, 1855, p. 350. — Eds. Prof. Simpson's Work.

Dr. Keiller stated that he had, a few months previously, met with an instance of the same disease, where the child's head at birth offered the same unusual presentation. I have lately examined the child, who is now about a year old, with Dr. Keiller. There is still a large, irregularly-lobulated swelling in the region of the thyroid gland, and stretching somewhat upward on each side of the trachea. It projects forward, and appears to swell out when the child cries. At other times, the skin of the neck looks flaccid, wrinkled and empty, over the site of the tumor, in consequence of the tumor itself having diminished and shrunk considerably since the time of birth. The lobulated masses of the tumor feel firm and hard to the touch; and probably the intervening and connecting tissue, in which the absorption has been specially marked, was originally more cystic in its character. The tumor does not seem to affect in any way the general health and growth of the child. The mother was born and brought up in the County of Cumberland, where goitre is not uncommon; but neither she nor any of her relatives were ever in the least degree affected by it. The present goitrous infant is the first child which she has borne. Before pregnancy occurred, she was under my care for chronic metritis; but her general health was good.

An instance of congenital cervical tumor, under the title of "*Scrofula in Fœtu*," was long ago described by Francus,\* with characters and a site which have made Grætzner and Montgomery refer it to the head of goitre. In this, as in Dr. Keiller's case, the certainty of the tumor consisting of enlargement of the thyroid gland was not made out by dissection.

*Case V.* — The child — a boy — presented at birth a tumor on both sides of the neck, but it was largest on the left. When the infant cried, or moved his neck too freely, that on the left side swelled excessively, and appeared to interfere with the power of suction and deglutition. Francus adds, that he unsuccessfully tried to effect the removal of the swelling by various remedies, local and general, and that notwithstanding it increased daily in size.

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\* *Ephemer. Nat. Cur.*, Dec. ii., An. v., Obs. 223.

## Editorial and Miscellaneous.

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### A SUGGESTION.

It is a matter of surprise that among many of the improvements that have been made in the Pharmacopœia, no admission has yet taken place for efficient solutions of many of the rarer and recondite alkaloids. It must have occurred to every practitioner, that in many instances he would have felt inclined to give aconitina, atropia, or some such preparation, if he could only give them in a sufficiently small dose, but knowing the difficulty, and in fact the impossibility of a druggist putting up such a prescription as the sixtieth or the hundredth of a grain, he is forced to abstain and give some milder or more gross preparation. It is no doubt owing to their having preparations of these remedies of a recognized strength, and put up with care and accuracy, that the Homœopathic practitioners owe much of the success which in many instances they have experienced. Even could we depend upon the accuracy of apothecaries, still very few men would like to burthen them in weighing out frequently remedies requiring to be given in minute fractional parts of a grain. It certainly would be a great accommodation both to practitioner, apothecary and patient, if some formula could be introduced into the Pharmacopœia, containing these remedies in such a condition as to be readily and easily procured. That the danger of prescribing these remedies as at present we are obliged to prescribe them is not entirely fanciful, is proved by the fact of Dr. Pereira stating "that owing to the great insecurity attending the internal use of aconitina, he has long ceased to employ it, as the slightest inattention on the part of the dispenser may be attended with fatal results." For aconitina and atropia, the menstruum, either alcohol or water, might be so proportioned as to contain in every five minims one two hundredth of a grain of the salt. This would require the solution of one grain of the salt in about two ounces of fluid. It is better to have the alkaloid highly divided, as it frequently occurs that these remedies have to be administered either to children or to highly excitable nervous people, who have a strong disinclination to take medicines. A powder might also be made with sugar of milk—say a grain of aconitina to every thousand grains of sugar of milk, so that every five grains will contain the one-two hundredth of the alkaloid. It ought to be strongly impressed upon the minds of all who have the charge of our Pharmacopœia, that for the safety of the public there exists an absolute necessity of making some provision of the kind, and we think it should be done without delay.

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UNIVERSITY OF LOUISIANA.—*Medical Department.*—The introductory course of lectures in this institution, for the session of 1855 and 1856, began on Monday, the 19th ult. The number of students present at the opening lecture was greater than usual, and judging from the list of matriculants thus far, the class promises to be the largest ever assembled within the college walls.



Occupying geographically a position which renders it easy of access, from the Southern and Western States, ranking with the first schools in the Union, possessing an anatomical museum unequalled in the country, and with hospital advantages incomparably the best, the future of this school cannot fail to be all that its most sanguine friends could desire.

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DR. O. W. HOLMES AND SPECIALISM IN MEDICINE.—In one of our late exchanges we notice an extract from some remarks made by Dr. Holmes on the occasion of the annual dinner of the Massachusetts Medical Society, on the subject of "Specialties" in medicine. Whilst we most cordially endorse some of the passing remarks of Dr. H., on the subject of the thorough education of physicians, we cannot help thinking that his tirade against Specialties must result in more harm than good to the profession.

Had we been ignorant of the name of the author of the article, we should nevertheless have promptly recognised Dr. H. in the unique style which characterizes it. For him to be witty is but to breathe. To borrow an idea of the noble and lamented S. S. Prentiss, wit and Oliver Wendell Holmes are but tautology in language. To accuse his brain of lavishing its wit, would be accusing Vesuvius of wasting her ever ascending smoke; the supply of the two is as inexhaustible as they are evanescent.

We have heretofore taken occasion to make a few observations in defence of Specialism in medicine, and as yet we see no reason why we should abandon our position. If an individual undertakes to be an oculist without having previously made himself thoroughly acquainted with anatomy, physiology and optics, he at once places himself on the list of quacks; but if he, after having acquired a good preliminary education, acquires such an amount of knowledge of the various branches of medicine as is required by our best medical schools, there can be no harm resulting from his thereafter devoting himself to the practice of any particular branch. On the contrary, such a course, if generally adopted and properly supported by the profession themselves, must result in great benefit to all parties concerned. Almost any well trained mind can master one subject, but very few can master *all*. If Dr. Holmes means to wage a war against the ignorant quacks who impose on every community as Specialists, he has our best wishes for his success in the undertaking; but if he also aims the blow at the thoroughly educated medical specialist—the oculist, aurist, obstetrician, surgeon, etc.—then we are forced to regard him as an unfortunately deluded, though honest enemy to the true interests of our common vocation.

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USE OF PHOSPHATE OF LIME.—Experiments have lately been made in the Bethany Hospital, in Berlin, with the phosphate of lime, recommended by Beneke, in cases affected with scrofulous diseases, caries of joints, infiltrated and suppurating lymphatic glands, spina ventosa, etc. In no case was any improvement noticed in the patients; on the contrary, an increase of the disease was observed in every instance. The preparation was used in doses of from two to four grains, repeated three times daily, and was continued without intermission for as much as six weeks

## HEALTH OF OUR CITY.

*List of Mortality for Four Weeks, ending Nov. 30, 1855.*

	1st w'k.	2d w'k.	3d w'k.	4th w'k.	Total.
Total number of Deaths,.....	94	76	64	69	303
Males, .....	63	43	35	41	182
Females, .....	21	28	24	24	97
Sex not stated,.....	10	5	5	4	24
Whites,.....	77	65	56	59	257
Blacks,.....	9	6	2	6	23
Mulattos, .....	4	1	6	4	15
Color not stated, .....	4	4	0	0	8
Native Americans,.....	28	9	4	20	61
Northern States,.....	3	2	0	2	7
Western States,.....	1	0	0	0	1
Southern States, .....	24	7	4	18	53
Foreigners, .....	45	26	25	27	123
English, .....	3	0	0	1	4
Irish,.....	14	14	13	14	55
French, .....	4	4	4	3	15
Germans,.....	11	7	3	4	25
Place of Birth not stated,.....	21	41	35	22	119
Age not stated,.....	15	10	15	4	44
Under one month old,.....	14	8	1	10	33
From one to five years,.....	12	15	10	16	53
From five to ten years,.....	2	2	3	2	9
From ten to twenty years,.....	6	2	3	1	12
From twenty to thirty years,.....	19	13	15	12	59
From thirty to forty years,.....	11	9	7	9	36
From forty to fifty years,.....	6	8	2	6	22
From fifty to sixty years,.....	2	2	3	4	11
From sixty to seventy years,.....	4	4	3	2	13
From seventy to eighty years,.....	1	2	2	2	7
From eighty to ninety years,.....	2	1	0	1	4
Over ninety years,.....	0	0	0	0	0
Yellow Fever,.....	6	7	2	3	18
Other Diseases, .....	73	61	53	52	239
Typhoid Fever,.....	1	1	1	3	6
Cholera, .....	2	2	2	3	9
Intemperance, .....	0	0	0	2	2
Accidental, .....	3	0	0	0	3
Still-Born, .....	5	3	3	3	14
Diseases not stated,.....	4	2	3	3	12

## CHARITY HOSPITAL REPORT — For the month of November, 1855.

Admitted, .....	862
Discharged, .....	798
Died, .....	79
Births, — Males, 5; Females, 7; Still-born, 1. Total, 13.	

LETTERS TO A YOUNG PHYSICIAN.—We have received from the publishers, Phillips, Sampson & Co., Boston, through our friend, Thomas L. White, No. 105 Canal street, a neat little volume under the above title. We have only had time to give it a cursory reading; but we do not hesitate to express our unqualified approval of the work, and recommend it to the attention of all young physicians. The name of the author, however, is sufficient to entitle it to the highest respect and attention. Dr. James Jackson is known and appreciated throughout the Union. The work is for sale at White's.

TO OUR SUBSCRIBERS.—The rapidly approaching end of the year, and the accumulation of bills for the expenses of our journal, force us to remind some of our patrons of their indebtedness to us. Now that our Southern rivers are rising, we are sure that the same boats which come freighted with the great staple, will bring us remittances from our subscribers; one follows the other, as a matter of course. Money is the great fly-wheel of our machinery; its weight must be felt, or everything goes to pieces.

Have we a subscriber who will refuse the little pittance he owes us? To us it is a very important item; to him it is nothing.

## Excerpta.

AN HISTORICAL SKETCH OF ORTHOPÆDIC SURGERY. — *By William Adams, F. R. C. S.*—Orthopædic Surgery has for its object the scientific treatment of deformities, congenital and non-congenital, whether affecting the feet, the hands, the spine, neck, eyes as in squinting, knees, hips, or other portions of the body. However dissimilar some of these affections may appear on their first mention, further observation will show their intimate relations to, and not unfrequently their mutual dependents upon, one another. Having frequently the same origin, depending upon the same laws, and affecting structures physiologically similar, they will be found to be amenable to the same principles of treatment.

The treatment of deformities was limited to mechanical means till the year 1784, when a physician of Frankfurt, named Thilenius, proposed division of the tendo-Achillis in the case of a young lady, aged 17, affected with talipes varus, which had resisted mechanical treatment. That operation was performed under his direction, on the 26th of March of the same year, by a Surgeon of the name of Lorenz. The heel immediately descended two inches, enabling the patient to tread on the entire sole. From this fact I infer that it was a non-congenital case. The cicatrization of the "large wound" was complete on the 12th of May, and the cure is said to have been perfect.

The operation of Thilenius and Lorenz was repeated by Sartorius on the 10th May, 1806, in a case of talipes equinus, in a boy aged 13, the result of abscesses on the back of the leg. These cases are related by Dr. Little in his work on Club-foot, (London, 1839.) A tourniquet having been applied to the femoral artery, a longitudinal incision, four inches in length, was made over the centre of the tendo-Achillis, the integument dissected off, and the fascia divided on a director to the same extent. The tendon was then cut across, and force employed to bend the foot. This failing, however, the incision was extended to the os calcis, the cicatrices divided, and the tendon isolated. Still the joint would not yield, and Sartorius tells us that he employed his entire strength, when the joint gave way with such a noise and cracking as if the whole of the bones had been broken. It is said that no fracture was discovered, but the operator was very uneasy concerning the probable consequences. Symptomatic fever, with suppuration, occurred, but the latter was not extensive. In nine weeks the wound cicatrized, complete ankylosis of the ankle having resulted; the patient was, nevertheless, able to walk with ease, unaided by a stick.

Such was the treatment of a case of talipes equinus fifty years ago! The result was more fortunate than might have been expected, or than would generally attend such a procedure.



In the year 1809, Michaelis, of Marbourg, in Germany, operated on several cases of talipes equinus, by partially dividing the tendo-Achillis; but, as he is said to have brought the feet at once into a natural position, it is presumed he either ruptured the undivided portion, or made complete sections.

Delpech, of Montpellier, next suggested an important modification in this operation, viz: a diminution of the external wound. He did not expose the tendon by dissection, but transixed the limb between the tendo-Achillis and the deep muscles with a common scalpel, the wound on the integuments on either side being an inch in length; he then introduced a convex-edged bistoury, and divided the tendon from before backwards, carefully leaving the bridge of skin over the space between the divided ends of the tendon. This operation he performed in May, 1816, on a boy, aged 9 years, affected with talipes equinus. Here we see the first step in advance towards the present subcutaneous method. But Delpech also conceived the idea that the divided ends of the tendon should be retained in apposition after the operation till union had taken place, after which, the uniting fibrous substance should be gradually and carefully extended till it assumed a degree of length equal to the shortened muscle. This was indeed a most valuable suggestion, and is the method we adopt at the present time, though by no means universally followed.

The rules laid down by Delpech for the performance of the operation, if taken verbally, appear to be much better than his practice.

He says — "1st. The tendon to be divided should not be exposed; its section should be accomplished by turning the instrument on one side, so that the line of incision may not be parallel to the division of the skin. Without this precaution, risk of exfoliation of the tendon is incurred."

From these words it might be inferred that Delpech performed the subcutaneous division of the tendon; but from his own account of the operation it is clear that his idea of not exposing the tendon was comparative in relation to the large incisions and complete dissection of his predecessors, and that he had no idea whatever of the true subcutaneous method. This great improvement was left for Stromeyer to accomplish. The other rules laid down by Delpech relate to the after-treatment, and are as correct in principle as if they had been written at the present day: in fact, precisely those upon which we at present act. They are as follows:

"2d. Immediately after division of the tendon, the divided extremities should be brought into contact, and kept in this position by suitable apparatus until their union is effected.

"3d. As reunion can only take place by an intermediate fibrous substance, which is capable of elongation prior to its solidification, it should be gradually and carefully extended to a length sufficient to compensate for the shortening of the contracted muscle.

"4th. When this degree of extension has been effected, the limb should invariably be retained in the improved position, until the new tissue has acquired the firmness of which it is susceptible."

It seems strangely inconsistent, that the Surgeon who could lay down these excellent rules, so well calculated to prevent disturbance of the parts after the operation, and favor the reparative process, should have accompanied his own operation with so much violence. In explanation, however, we at once recognize the absence of Pathological observation — the true basis of scientific Surgery.

And yet it may be a proud reflection for us to know, that the great pathological law, deduced directly from accurate pathological and surgical observations, which should have determined the practice in this case, and a knowledge of which would have enabled Delpech to have carried off more than the full meed of praise which his warmest admirers have awarded him, had many years previously been published in the works of the illustrious Hunter.

In his "Treatise on the Blood, Inflammation, and Gun-shot Wounds," published in the year 1794, twenty-two years, mark you, before Delpech performed his operation, and thirty-seven years before the imperfectly subcutaneous operation of Stromeyer, Hunter points out as a great fundamental principle, in reference to the healing of injuries, the difference between those two forms of injuries, of which one is subcutaneous, the other open to the air. He says, "The injuries done to sound parts, I shall divide into two sorts, according to the effects of the accident.



The first kind consists of those in which the injured parts do not communicate externally, as concussions of the whole body, or of particular parts, strains, bruises, and simple fractures, either of bone or of tendon, which form a large division. The second consists of those which have an external communication, comprehending wounds of all kinds, and compound fractures. Bruises which have destroyed the life of the part may be considered as a third division, partaking, at the beginning, of the nature of the first, but finally terminating like the second. The injuries of the first division, in which the parts do not communicate externally, seldom inflame, while those of the second commonly both inflame and suppurate." The deviations from this law in particular instances are then adverted to. Here, then, is the law of the reparative process in these two great classes of injuries. In these sentences, observes Mr. Paget, in his *Lectures on Surgical Pathology*, "Hunter has embodied the principle on which is founded the whole practice of subcutaneous Surgery; a principle of which, indeed, it seems hardly possible to exaggerate the importance." With truth, indeed, has it been said, that those who trace the progress of modern Surgery to its source, will not fail to discern in the principles which Hunter established, the germs of almost all the improvements which have been since introduced.

Whether Hunter ever divided tendons subcutaneously may perhaps be somewhat doubtful, but it is believed that he did. In the Museum of the Royal College of Surgeons there are five specimens of Achillis tendons from the ass and deer, divided by Hunter, for the purpose of investigating the process of repair; and in the descriptions of three of these specimens in the *Pathological Catalogue*, Vol. II. Nos. 349, 351, and 352, it is stated, "The tendon was divided transversely, and, it is believed, by subcutaneous section." In one of the other two specimens, No. 348, the experiment was performed by open wound; and in the other, No. 354, it is stated that the operation was "probably not by subcutaneous incision."

It has been generally regretted that Delpech's first case was so unfortunate as to prevent him repeating the operation with such modifications as further reflection and experience would have offered; but he was ignorant of the true pathological basis upon which alone such operations can be conducted with safety to the patient, though it had been clearly enunciated by Hunter, twenty-two years previously, it may be as well that further experiments were not made. When he wrote his celebrated work, "*L'Orthomorphie*," thirteen years after this operation, he did not recommend any modification of his mode of procedure.

From the year 1816, in which Delpech's operation was performed, to the year 1831, when Stromeyer divided the tendo-Achillis upon a greatly improved method, though not quite subcutaneously, Dr. Little states that there are no recorded repetitions of the operation.

In the month of February, 1831, Stromeyer, of Hanover, divided the tendo-Achillis, and in 1833 and '34, published two memoirs detailing the history of six successful cases. From this period we may really date the commencement of Orthopædic Surgery. "To Stromeyer," observes Dr. Little, "is due the honor of establishing the division of tendons on a secure and permanent basis, and of insuring its reception as a standard operation in the art of Surgery." Stromeyer not only fully appreciated the value of the rules laid down by Delpech, and the soundness of the principles on which he recommended that the subsequent treatment should be conducted, but he also recognized the great source of danger in his predecessor's method of operation, viz: the large external wound, and disturbance of parts. In this respect, therefore, Stromeyer improved the operation. He operated by "puncture," as he expresses it, without external incision. He used a small bistoury, but still he transfixed the limb between the tendons and the deep muscles, making two small wounds, and effected division of the tendon, by pressing it against the edge of the knife, rather than cutting it by any movement of the knife, which he considered would have endangered an increase in size of the cutaneous wounds. Stromeyer describes his mode of operating in his great work on *Operative Orthopædic Surgery*, and his account is translated by Dr. Little.

Stromeyer remarks, "I have frequently divided the tendo-Achillis in this manner without producing a second puncture; but this is of little moment, as two minute punctures heal as quickly as a single one." Here, then, is the true subcu-

taneous section described in the year 1838, in juxtaposition with the plan of transfixing the limb, which Stromeyer preferred for the following reason. "The performance of the operation with the point of the instrument is less to be relied on, partly from its being too weak, and also, because the operator can be less certain of not causing injury to other structures in the event of the patient not remaining quiet throughout the operation." The preference given to the plan of transfixing the limb is remarkable; but the number of cases so operated upon proves to us, on sufficient evidence, that it is a proceeding generally free from risk when the wounds are small. I have myself seen it done accidentally two or three times without exciting inflammation, but, at the same time, I think its adoption by Stromeyer goes far to show that he sought to improve the operation, by diminishing what he correctly conceived to be the great defect in the operations of his predecessors, rather than to establish it on the broad basis of the pathological law pointed out by Hunter as regulating the reparative process of "injuries in which there is no external communication," above adverted to.

Nevertheless, whether this result was arrived at as a mere practical suggestion, looking to a diminution of what appeared to be the source of failure in the method of his predecessors, or as a scientific deduction from pathological and surgical observations, the credit still belongs to Stromeyer of having re-introduced this operation, and performing it in the only way in which it can be undertaken with safety to the patient. Stromeyer soon had the gratification of witnessing the splendid results of this operation, which he had deprived of all its terrors. He had to contend with the strongest opposition and the severest criticism from his contemporaries, but these he overcame by his determined perseverance, and by a multitude of cases was enabled to demonstrate the safety of the operation. Dr. Little states that it was two or three years before his example found imitators. Dieffenbach, of Berlin, appears to have been among the first and most energetic followers of the Stromeyerian operation. Dr. Little states that in 1839, Dieffenbach had, up to that period, divided the tendo-Achillis in upwards of 150 cases of distorted feet. Bouvier of Paris, Poli in Germany, Duval, who first divided the tendo-Achillis in Paris, in 1835, Jules Guerin of Paris, Bonnet of Lyons, and Scoutetten of Strasburg, also quickly adopted it.

Now, I think it very likely to occur to you to ask me why, in tracing the origin and development of Orthopædic Surgery, I refer so much to the history of the subcutaneous operation in club-foot, when we know the treatment of every other deformity of the body; wry-neck, contractions of joints, curvatures of the spine, and long bones, squinting, etc., are generally included in this branch of Surgery. My reply is, that the art of curing deformities rests its claims to be ranked and incorporated with the general science of Surgery, almost exclusively upon the perfection of this operation, which I may confidently state, without fear of contradiction, to be, in reference to the extent of its application, and the benefits it has conferred upon many thousands of our afflicted fellow-beings, the greatest improvement in modern Surgery. In saying this, I am by no means unmindful of the numerous and important improvements in modern Surgery which have been suggested and perfected by the genius and industry of our professional brethren both abroad and at home, many of them arrived at after a much larger expenditure of mental labor: but a reference to the statistical tables I shall bring before you, of 17,000 cases of deformity, which have been treated at the Royal Orthopædic Hospital; and the knowledge that, probably two or three times that number have been treated on the Continent, some of the statistical results of which I shall also be enabled to bring before you, will amply bear out the assertion I have made in claiming for subcutaneous tenotomy the highest rank among the practical and scientific improvements of modern Surgery. — *Virginia Med. and Surg. Journal.*

CLINICS—*Statistical Report of the Principal Operations performed in the London Hospitals, during the month of April, 1855.*

*Lithotomy.*—Number of cases, 3; recovered, 1; under treatment, 2.

*Case 1.*—A man, aged 20, under the care of Mr. Lloyd, in St. Bartholomew's Hospital. The recto-urethral operation was performed, and a stone of considerable size removed. Doing well. *Case 2.*—A boy, aged 3, under the care of Mr.

Lloyd, in St. Bartholomew's Hospital. In this case, also, the recto urethral method was adopted. Doing well. *Case 3.*—A boy aged 9, under the care of Mr. James Lane in St. Mary's Hospital. A small mulberry calculus was removed by the usual lateral operation. Recovered.

*Lithotrity.*—A man, aged 28, asthmatic, and a bad subject for an operation, is under Mr. Hilton's care, in Guy's Hospital, with stone in the bladder. His symptoms dated three years back, and he had suffered severely. The bladder is very irritable, much contracted, and has never been got to hold more than four ounces at a time. Two lithotrity operations have been performed, and numerous fragments have come away. Considerable irritation has been caused, but on the whole the man is doing well.

*Ligature of Arteries.*—Mr. Stanley's patient, in whom ligature of the femoral had been practiced for popliteal aneurism, has been discharged. He is well in every respect, excepting that a collection of fluid of doubtful nature still remains in the aneurismal sac, and appears to be absorbing very slowly. He has for some time been allowed to take exercise. Mr. Cock's patient, in whom a like operation had been performed, has been discharged, under almost similar circumstances. The fluid in the sac was, however, diminishing more rapidly than in Mr. Stanley's case.

*Case 1.*—A muscular draymen, aged 28, was admitted into St. Bartholomew's Hospital, under the care of Mr. M'Whinnie, having had his left leg severely crushed between his dray and a gate post. The whole limb below the knee was swollen, tense, and cold, and the swelling was said to have commenced immediately after the accident. Four hours after admission the swelling had much increased, more especially in the popliteal space. As no pulsation had been detected in either tibial artery, it was now deemed probable that rupture of the main trunk had occurred, and Mr. M'Whinnie accordingly determined to cut down into the popliteal space and examine. The popliteal and anterior tibial arteries were found on exposure to be uninjured: but the dissection being continued downwards, it was ascertained that the posterior tibial, about an inch from its origin, was collapsed and pulseless. No laceration was discovered at the time: but, as it was thought certain it must exist, two ligatures were applied, one below and one above the spot where pulsation ceased. No bleeding occurred after the operation, but the patient sank, became delirious, and death took place on the day following. At the *post-mortem* there was found, as had been suspected, a laceration in the posterior tibial artery, just below the spot where the ligature had been applied. All the other vessels were sound. *Case 2.*—A man, aged 25, admitted into Guy's Hospital, under the care of Mr. Callaway, having injured his fore arm by the bursting of a soda-water bottle. There had been profuse arterial hemorrhage from a wound just above the wrist. Mr. Callaway cut down upon and tied the radial artery at two parts, above and below a transverse injury, which had nearly divided its trunk. After this had been done, there seemed to be still bleeding from a branch near to the radial, supposed to be the *superficialis volæ*. This also was tied, and the bleeding then ceased. No tendons had been injured in the accident. The wound healed kindly.

*Herniotomy.*—Number of cases, 6; recovered, 2; under treatment, 3; died, 1

*Recoveries.*—*Case 1.*—A man, aged 53, under the care of Mr. Hilton, in Guy's Hospital, with a large inguinal hernia, which had been strangulated eight hours. It was found necessary to open the sac, but the stricture seemed to have been caused by a portion of the fascia propria. A large mass of omentum and a small coil of intestine occupied the sac; the latter was returned unseen. After the operation the man had acute inflammation of the sac, and also some peritonitis. Under treatment, however, by calomel and opium, he made a good recovery. *Case 2.*—A woman, aged 40, under the care of Mr. Quain, in University College Hospital, hernia femoral, of very small size; strangulated two days; symptoms not very acute. The sac was not opened. Recovered. *Case 3.*—A woman, aged 33, under the care of Mr. Erichsen, in University College Hospital. Hernia femoral; strangulated seven hours; sac not opened. Recovered. Under Treatment.—*Case 4.*—A woman, aged 28, under the care of Mr. Spencer Smith, in St. Mary's Hospital, the subject of femoral hernia for six years. At the time of admission the protrusion had been down twelve hours, and there had been vomiting during that



time. The bowels had, however, acted that morning under the influence of a dose of castor oil. Attempts at taxis proving ineffectual, the operation was at once performed. The sac having been opened, a knuckle of much congested intestine was brought into view. On the division of Gimbernat's ligament an artery of some size was wounded, and it became necessary to apply a ligature to it. There were the symptoms of sharp peritonitis after the operation, but they subsided under treatment, and the patient is now doing well. *Case 5.*—A woman, aged 42, under the care of Mr. Ure, in St. Mary's Hospital, for years the subject of reducible femoral hernia. Strangulation had existed about nine hours at the time of the operation. Sac opened. An abscess formed after the operation above the posterior part of the crest of the ilium. Doing well. Deaths.—*Case 6.*—A woman, aged 57, under the care of Mr. Birkett, in Guy's Hospital. Hernia femoral; strangulated fifty-seven hours; sac opened. The bowel was actually inflamed, and almost gangrenous; but not so hopeless but that reduction was deemed warrantable. On the fourth day a faecal fistula formed, and death from exhaustion resulted on the fourteenth. No autopsy was permitted.

*Amputations.*—*Of the Thigh.*—Under treatment.—*Case 1.*—A man, aged 25, under the care of Mr. Hilton, in Guy's Hospital, on account of necrosis of the tibia and disease of knee-joint. He was the subject, also, of diseased hip, on the same side. His health was bad, and there were signs either of pneumonia or tubercular consolidation of a portion of one lung. He had been in the hospital for nearly nine months; and, as the disease in the knee seemed to be rapidly exhausting him, it was thought best to remove the limb. The amputation was performed through the middle of the thigh. On the tenth day secondary hemorrhage occurred, and the stump had to be laid open, and a second ligature applied to the femoral artery, in which vessel an ulcerated opening was found. The case is now doing well. *Case 2.*—A woman, aged 23, under Mr. Hilton's care, in Guy's Hospital, on account of ankylosis of the knee-joint. The disease had existed for eighteen months, and had much reduced her health. Amputation by double flaps through the thigh. Doing well. *Case 3.*—A strumous boy, aged 3, under Mr. Hilton's care, in Guy's Hospital, with necrosis of the tibia and ankylosis of the knee-joint. Amputation through the thigh. Doing well. *Case 4.*—A boy, aged 12, weak and much emaciated, under the care of Mr. Cowland, in the London Hospital, on account of necrosis of the tibia and disease of the knee-joint. Flap amputation in the lower third was performed. Doing well. *Case 5.*—A man, aged 56, under the care of Mr. Hancock, in Charing-Cross Hospital, on account of diseased knee-joint and abscesses in the leg. His state of health at the time of amputation was very bad, but he has borne it well, and is now progressing favorably. Deaths.—*Case 6.*—A man, aged 57, under the care of Mr. M'Whinnie, in St. Bartholomew's Hospital, for gangrene in consequence of a severe crush of the leg. He was only admitted on the fourth day, when the mortification had already extended high up, and his state was extremely critical. Death followed twelve hours after the amputation. *Case 7.*—A man, aged 60, of very intemperate habits, was admitted into St. Mary's Hospital, under the care of Mr. Ure, having sustained a compound-fracture of the femur in its lower third, with extensive laceration of the soft parts. He was intoxicated at the time of the accident, which had occurred six or seven hours previous to his admission. Primary amputation was performed. Gangrene afterwards attacked the stump, and death occurred on the fifth day.

*Of the Leg.*—Under Treatment.—*Case 8.*—A man, aged 34, under the care of Mr. Cock, in Guy's Hospital, on account of old standing disease of the tarsus, for which several operations had been previously performed. He had been two years in the hospital, and, as the processes of repair proceeded very slowly, it was at length thought necessary to amputate. A double flap operation was performed in the middle third of the leg, the flaps being cut from without inwards. The tibia was found to have undergone extreme induration, its cancellus tissue having been replaced by bone almost as hard as ivory. The stump has done well, and is now healed, excepting a single sinus, which depends upon a portion of bone which is dead, and will have to exfoliate.

*Of the Upper Extremity.*—Recovered.—*Case 9.*—A healthy lad, aged 20, under the care of Mr. Fergusson, in King's College Hospital, on account of extensive la-



ceration of the hand by machinery. An attempt was made to save it, but gangrene came on, and it was necessary to amputate through the forearm on the third day. Doing well. *Case 10.*—A laborer from the country was admitted into Guy's Hospital, under the care of Mr. Birkett, his right hand and wrist having been crushed in the wheels of a threshing machine. Primary amputation was performed. The stump healed by granulation, and was sound in about three weeks.

*Of the Foot.*—*Death.*—*Case 11.*—A sallow-complexioned man, aged 27, under the care of Mr. Fergusson, in King's College Hospital, on account of caries of the tarsal bones, the result of an injury. Amputation at the ankle-joint was performed. After the operation, extreme irritability of the stomach came, and violent sickness continued unchecked nearly to the time of death, which took place on the sixth day. The flap would probably have sloughed. Under treatment,—A man, aged 56, under the care of Mr. Cock, in Guy's Hospital, on account of disease of the ankle-joint of one year's standing. The skin over the heel was so much diseased, that the ordinary flap, in amputation at the ankle-joint, could not be obtained; Mr. Cock therefore dissected up the skin very freely from the front and outer part of the articulation. The lower surface of the tibia and the head of the fibula were sawn away. Considerable portions of the flaps sloughed. and the stump was, for the first week, in a very unhealthy condition; it has latterly, however, much improved, and it is hoped the result may yet be good.

*Excision of Bones and Joints.*—Several cases mentioned in previous reports remain under care. Mr. Statham's patient in the case of excision of the knee-joint, performed about nine months ago, has been discharged from the hospital. The sinuses are mostly healed, but she is still very weak, and unable to sit up. During the month the following have been performed: *Case 1.*—A strumous woman, aged 27, under the care of Mr. Fergusson, in King's College Hospital, on account of diseased elbow-joint. A complete excision of the articulation was performed, the H-shaped incision being adopted. Doing well. *Case 3.*—A strumous child, aged 7, under the care of Mr. Hilton, in Guy's Hospital, on account of disease of the os calcis and astragalus. The os calcis was not actually necrosed, but was so extensively diseased that it became necessary to remove the whole of it. A semilunar flap was dissected up from the outer side of the heel, and the os calcis, thus exposed, was cut in two by bone forceps. The periosteum, where it adhered, was next carefully detached by the handle of the knife, and the two halves of the bone were easily removed. The under surface of the astragalus being carious was gouged out. Hitherto the case promises well. It is hoped that, as the periosteum was carefully preserved, there may be a considerable production of new bone. *Case 3.*—A woman, aged 25, under the care of Mr. Hilton, in Guy's Hospital, on account of diseased os calcis. The skin was dissected up over the affected spot, and the gouge freely applied. The wound has healed, excepting a single fistula, which, it is feared, leads to bone yet in a state of disease; the inflammation not improbably involving the articulation between the astragalus and os calcis. *Case 4.*—An unhealthy man, aged 32, under the care of Mr. Stanley, for disease of the carpal bones. A semilunar flap was dissected up from the back of the hand, and most of the carpal bones, loosened and in a state of caries, taken out. Doing well. *Case 5.*—A man, aged 52, under the care of Mr. Paget, in St. Bartholomew's Hospital, on account of old standing disease of the os calcis. In another hospital five operations with the gouge, trephine, etc., had been performed, but the remaining portion of bone still continued the diseased action. Mr. Paget excised all that remained of the affected bone, and afterwards divided the tendo-Achillis (subcutaneously) in order the better to permit of apposition. The wound made was, from the chronic thickening of the parts, etc., necessarily very large; but it progressed well, and the result seems likely to be favorable. *Case 6.*—A healthy man, aged 31, under the care of Mr. Fergusson, in King's College Hospital, on account of a tumor in the left side and front of the lower jaw. It was necessary to excise the bone from the anterior edge of the left masseter as far forwards as the canine tooth on the right side. The disease had been increasing for three years. On dissection, the bone was found occupied by cysts filled with gelatinous fluid, its external layer having been expanded until as thin as parchment. The patient has done well since the operation. *Case 7.*—A man, aged 55, under the care of Mr. Birkett, in Guy's

Hospital, having suffered from carious disease within the head of the tibia for five months. The gouge was employed, and some softened cancellous bone infiltrated with pus was removed. The cavity seems to be filling up.

*Removal of Necrosed Bone.*—Many cases of this class of operations remain under care in the different hospitals. In some, further operative measures will be required at a future time; others are likely to recover without.

The following have been performed during the month: *Case 1.*—A boy, aged 11, under care in Guy's for necrosis of the right tibia. The disease had existed for three years. Mr. Hilton removed a very large sequestrum, and it is believed all the affected portion. The wound has since been attacked with erysipelas, but is now healing. *Case 2.*—A lad, aged 17, under Mr. Lane's care in St. Mary's Hospital, having twice before had necrosed bone removed from the humerus. A third operation has been performed. Doing well. *Case 3.*—A man, aged 38, under the care of Mr. Adams, in the London Hospital. He was in good health when admitted, but for twenty-eight years had suffered from an ulcer in front of the left tibia which depended upon diseased bone. The tibia was much thickened about the part. Two attacks of mild hospital gangrene affected the wound, and caused delay of the operation. The tendency to phagedæna having, however, quite ceased, Mr. Adams proceeded to the removal of the necrosed portions. This was effected successfully, though not without some difficulty on account of the great thickness and density of the bone by which they were encased. Under treatment. *Case 4.*—A girl, aged 19, under the care of Mr. Birkett, in Guy's Hospital, on account of necrosis of a small part of the shaft of the ulna. The disease had existed four months. Erysipelas attacked the wound a few days after the removal of the sequestrum had been performed. Recovered.

*Excision of Malignant Growths.*—*Case 1.*—A man, aged 72, under Mr. Cock's care in Guy's Hospital, on account of a returned epithelial cancer of the lip. In November last, Mr. Cock removed by a horizontal incision a superficial cancerous ulcer from the same spot. It had been necessary also to apply the actual cautery to several other growths of threatening character on the tongue and mucous lining of the cheek. Mr. Cock on the present occasion, excised the returned disease in the lip freely by the V-shaped incision. Recovered. *Case 2.* A woman, aged 36, married, and the mother of a large family, under the care of Mr. Birkett, in Guy's Hospital, on account of infiltrated cancer of the right breast. The entire gland was excised. Erysipelas followed the operation, but it subsided under treatment, and the wound is now healed. *Case 3.*—A woman, aged 37, under the care of Mr. Lane, in St. Mary's Hospital, for a tumor of the parotid the size of an orange. There was no history of hereditary tendency to cancer. The disease was said to have commenced as a small pea-sized mass about fifteen years ago, and had remained nearly stationary until within the last four months, during which it had been rapidly increasing. Mr. Lane excised the whole, and on cutting through the mass, it was found to be of encephaloid character in parts. The wound was affected by erysipelas after the operation, but subsequently it healed well. No paralysis of the face was caused.

See also "Excision of the Testis," and "Amputation of the Penis,"

*Excision of the Testis.*—A cachectic man, aged 51, under the care of Mr. Fergusson, in King's College Hospital. The left testis was enlarged to the size of a small cocoa-nut. After excision the disease proved, as expected, to be medullary cancer. Recovered.

*Amputation of the Penis.*—A man, aged 46, in good health, under the care of Mr. Hilton in Guy's Hospital, on account of epithelial cancer affecting both the prepuce and glands. The disease was of five months' standing. Mr. Hilton amputated the penis by transfixing its body just above the urethra, cutting through the copora cavernosa, and then dissecting out the urethra and dividing it half an inch farther forward than the first incision. The result has been very good, and the man has not experienced any difficulty in the escape of the urine.

*Excision of Non-Malignant Tumors.*—*Case 1.*—A boy, aged 9, under the care of Mr. Cock, on account of a pedunculated encysted tumor in the umbilicus. The tumor was of congenital origin, and had attained the size of a marble. It had been mistaken for a hernia, and a truss had been ordered. Mr. Cock at once cut it

away. Its contents consisted of sebaceous matter. *Case 2* — A woman, aged 24, under the care of Mr. Birkett in Guy's Hospital, on account of a mammary glandular tumor of four years' growth. The mass was about the size of half an egg, and quite circumscribed. Enucleation was performed. Recovered. *Case 3*. — A healthy woman, aged 23, under the care of Mr. Fergusson, in King's College Hospital, on account of an elastic painless tumor in the left lobe of the thyroid gland. Excision was performed. It proved to be a cyst filled with thick serous fluid, and attached to the capsule of the gland. Recovered. *Case 4*. — A woman, aged 28, under the care of Mr. Lane in St. Mary's Hospital, on account of a much enlarged and almost solid bursa over the patella. It was excised, and all went on well until the wound was almost healed, when suddenly the patient became affected with extreme mental depression, avowed her conviction that she should not recover, sank into low fever, and in the course of a few days died. *Case 5*. — A healthy man, under Mr. Lloyd's care, in St. Bartholomew's, for a large fatty tumor in the side. Excision. Recovery.

*Operations for Exostosis.* — *Case 1*. — A woman, aged 28, under the care of Mr. Cock, in Guy's Hospital, on account of what was considered to be a hard epulis, growing from the left side of the lower jaw. It was of six years' duration, and about the size of half a small walnut. Mr. Cock cut it freely away to a level of the jaw, and took out the stumps of two dead teeth, which were found in its base. Its structure was that of hardish bone, its formation having, no doubt, been caused by the irritation of the decayed teeth. The latter had been completely encapsuled by the growth. *Case 2*. — A man, aged 19, under the care of Mr. Hilton, in Guy's Hospital, on account of an exostosis, the size of a small walnut, growing from the inner side of the right femur, just above the knee-joint. Great care was necessary to avoid injury to the joint. The lowest edge of the vastus internus lay over the growth. Mr. Hilton divided the skin, muscles, etc., by a single incision, and with bone forceps cut through the base of the tumor. The wound has since been affected with unhealthy suppuration, but the case is otherwise doing well.

*Ligature of Nævus, etc.* — In four cases under the care, respectively, of Messrs. Fergusson, Erichsen, Cock, and Hilton, the usual operations by ligature, for small nævi on the scalp, have been successfully performed.

*Operations for Urethral Stricture.* — An intemperate man, aged 37, under the care of Mr. Partridge, in King's College Hospital, on account of stricture of the urethra, and perineal fistulæ. The stricture was of old standing, and had followed gonorrhœa. Mr. Partridge performed perineal section of the diseased tract in the usual way. Under treatment.

*Tracheotomy.* — A child, aged 3½ years, was admitted under the care of Mr. Birkett, into Guy's Hospital, having attempted to drink boiling water from the spout of a kettle. The trachea was opened, on account of very distressing dyspnoea, about seven hours after the accident. During the operation, the child all but died from suffocation. During the whole week it progressed very favorably, and appeared to be quite out of danger, when rather suddenly the signs of inflammation of the lungs set in. Death occurred on the fourteenth day. The *post mortem* showed acute pneumonia.

*Plastic Operations. — Hare-lip.* — Two cases, under the care of Mr. Fergusson, in King's College Hospital, the patients aged respectively 4 years and 6 months; one under that of Mr. Erichsen, in University College Hospital, aged 6 years; one under that of Mr. Cock, in Guy's, aged 6 months; and one under that of Mr. Hilton, in Guy's, aged 5 months, all examples of the single deformity, have been operated on in the usual way. In all, the success has been complete. In one case of double hare-lip, under Mr. Erichsen's care, in an infant aged 10 months, the usual operation has also been successfully performed. Mr. Erichsen did not, in either of the cases under his care, employ pins, using sutures only.

*Taliacotian.* — A case is under the care of Mr. Henry Thompson, in the Marylebone Infirmary, in which a new nose has been made by transplantation from the skin of the forehead. The result promises to be very good. The columna is to be made in the course of a few weeks.



*The Closure of Urethral Fistula.* — In a case under the care of Mr. Erichsen, in University College, in which a fistulous opening into the penile urethra had remained after a sloughing chancre, a plastic operation has been performed for its closure. No success has, however, attended it, and it is proposed to operate again shortly. — *N. Y., Med. News and Hospital Gazette.*

THE PATHOLOGY OF DELIRIUM TREMENS, AND ITS TREATMENT WITHOUT STIMULI OR OPIATES. — *By Dr. Peddie, of Edinburgh.* — Dr. Peddie holds that delirium tremens is a form of *alcoholic poisoning* — or an alcoholism — that it is specific in its nature, and that it is analogous to plumbism, mercurialism, egotism, or narcotism; and he considers, as entirely erroneous, the opinion that the privation of an accustomed stimulus is the exciting cause of the malady.

"Analogy," he says, "will not bear out this theory. Mercurial fumes, or the oxides of mercury, when long inhaled or absorbed into the body, as in the case of gilders, quicksilver-mines, and others, in the course of time produced an attack of shaking paralysis — the *tremblement mercurial* of the French pathologists; but will it be averred that the workmen long exposed are more likely to be affected with tremors, if removed from this poisonous atmosphere and occupation, than if they continued at their work? The reverse is well known to be the fact, not only in the case of such artisans, but of those also who are beginning to suffer in a somewhat similar way from lead poisoning. In both affections, when the symptoms are recent, a cure can only be effected by removal from the injurious occupation; otherwise the symptoms deepen with hourly increasing rapidity, until tremors are succeeded by sleeplessness, delirium, and ultimately coma."

The history of delirium tremens, in Dr. Peddie's opinion, is equally opposed to the idea that the disease is caused by the privation of a stimulus.

In a word, Dr. Peddie holds that the *exciting* as well as the predisposing cause is the habitual abuse of intoxicating liquors; that these produce a specific form of irritation of the brain and membranes, the tendency of which is to arachnoid inflammation; that the chief phenomena attending this disease are invariably uniform in their character, and distinguish it from every other affection; that the occurrence of the salutary sleep is the normal termination of the paroxysm, indicating diminished activity of the cerebral circulation and functions, and the commencement of convalescence; that the cordial and opiate treatment is generally pernicious, and frequently dangerous; and that the main indications of cure are, to reduce the cerebral excitement by a moderate but decided and steady course of antimony, or other agent capable of exerting a somewhat similar influence, and thus favor — not force — the wished-for sleep, to soothe the feelings and dissipate the fears of the affected by kind and judicious superintendence, and the permission of light and liberty, and to support the physical strength by a moderate allowance of animal nourishment.

Dr. Peddie's experience in the treatment of delirium tremens has been considerable. He has treated during the last ten years, "upwards of eighty cases of the genuine disease, many of them severe ones, with uniform success;" and in the paper under notice he cites 6 of these cases in illustration. Of these, the subjoined will serve as an example:

*Case.* — Mr B., æt. 48. spirit dealer. Long an habitual drinker. His average daily amount for some time had been four gills of whiskey and one bottle of beer, taken from early in the morning until late at night; and there had been no diminution in the quantity previous to the present seizure. Had slept very little for a week, and none at all on the last two nights; and for some days was very tremulous, and quite unable to transact business.

*1st day's visit,* 3 p. m. — Was very distressed and agitated during the last night, walking constantly up and down through the house, terrified with visions; had his last glass of whiskey at 11 this forenoon. Pulse 104, small; skin cool and clammy; great muscular tremor; tongue foul: eyes yellow and lustreless; mind constantly occupied with false and horrific impressions of all kinds, although in no very definite form; but can answer a question put directly to him. *Instructions* — Plenty of light; complete liberty to promenade through the house, the doors and windows being secured; and two intelligent men to attend and humor



all his fancies. To have a wine-glassful of the following mixture every two hours : ℞ Tart. Ant. gr. iv, Infusi Quassia et Aqua aa  $\mathfrak{z}$ x, whether it caused sickness or not, and only to be discontinued if he should go to sleep. Beef tea and coffee with milk to be given occasionally. 8 P. M. — Took one glass of the mixture at 3.30 P. M., which caused vomiting of a quantity of bilious matter; one at 5 o'clock, which was followed soon after by a loose alvine evacuation; and one at seven o'clock. He is at present pale and perspiring; very tremulous and restless — in constant apprehension of rats and strange men; quite sensible when spoken to; pulse 110. To have the mixture only every third hour. Beef tea, etc.

2d day, 10.30 A. M. — Pulse 106, very small; perspiring freely; face very pale; urine scanty and high colored; great tremulousness. He can put out his tongue, or rise up, or sit down when desired, but that is nearly the amount of his intelligence. He is in constant motion, not rapid or boisterous, but chiefly busy in arranging bed-clothes, carpets, small articles of furniture, and sweeping imaginary crumbs from off the table. Had never been in bed, and had taken only three doses of the mixture since I saw him last. Took a glass from me, supposing it to be pale brandy: — no sense of taste. The mixture to be continued regularly. Was seen by my friend Dr. Cappie, at 3 P. M., and again at 9 P. M., who found him much the same as when last reported. Had been purged several times. Antimony, etc., continued.

3d day, 2 P. M. — In bed, sound asleep; pulse 84, of good character; a good deal of subsultus tendinum; skin very moist; paleness of countenance gone. It was stated that he had appeared very much exhausted last night about 12 o'clock; was then got to bed, fell asleep almost immediately, and did not awake until 7 this morning. When awake he was not quite sensible, but took some bread, coffee and milk, and fell asleep again. Continued so for other two hours, and was then perfectly coherent, but not inclined to speak. He had some more breakfast and an egg, and went to sleep again. An hour ago he was awake for a few minutes, and took some beef tea. The antimony had been given once this morning: — to be discontinued. Nourishment only to be offered when he awakes.

4th day. — Read him quite will; mind perfectly clear, and had been able to read a little. — *Edinburgh Monthly Journal*.

TREATMENT OF PSEUDO-MEMBRANOUS ANGINA\* BY THE ALKALINE CARBONATES. — The solvent action attributed to the alkalis, upon the fluids of the body, and particularly upon certain abnormal products of secretion, has already been taken advantage of in quite a large number of diseases. We are not sure whether pseudo-membranous angina figures among the great variety of affections to which this treatment has, of late years especially, been applied; our recollections on this point are not exact. Were this so, however, the interest which attaches to the case recently communicated to the Academy of Sciences, by M. Marchal (de Calvi,) would be none the less real. Its clinical and practical bearing gives it naturally a place in our pages.

Starting with the idea that the principle which is the cause of the disease known under the name of pseudo-membranous angina (*angine couenneuse*) is unknown to us, but manifests itself by a phenomenon, the formation of false membranes, indicating an excess of plasticity, if not the immediate cause, the most striking phenomenon of the pathology of the malady, at least the fact which nearest approaches it, and to which, consequently, we must address ourselves, in order to attack the evil as nearly as possible to its source, M. Marchal, had for a long time, he says, conceived the idea of restoring in such cases to the employment of alkalis, without, at the same time, neglecting the inflammatory indications. This he has done successfully in the following case, which we give in his own words, adding that we adopt the conclusions of our *confrère*, with the reservations which he has made.

\* The French expression *angine couenneuse*, is one which it is difficult to render satisfactorily in English, but the literal translation, which we have adopted, will be sufficiently intelligible. The disease is also called — pseudo-membranous inflammation of the fauces, membranous angina, diphtheritis, etc. The latter synonym was introduced by Bretonneau, by whom the precise nature of the disease was first satisfactorily made known. — (TRANSLATOR.)

"M. B., Chief Engineer of the Vincennes Railway, was attacked, in the beginning of March, 1855, with a sore throat, which at first appeared slight, but which rapidly grew worse. When first called, at the beginning of the attack, I prescribed simple remedies. The next day, the inflammation was much more intense; the mucous membrane of the posterior fauces was very red and œdematous; the suffering very acute both in the posterior fauces, and in the sub-maxillary regions, and much increased by the act of deglutition. But what especially struck me, and from the first glance caused me the greatest uneasiness, were streaks of a pearl-white color on the surface of the tongue, and more particularly on the palatine mucous membrane, and that of the tonsils (which were not much swollen,) forming by their agglomeration very evident patches, concerning which there could be no mistake. It was, in fact, the product of a plastic exudation; only, in the mucous membrane of the throat, the product was interstitial, in other words, it had not passed through the epithelium; whereas, on the tongue, the false membranes, one of which was of the size of the nail of the little finger, were superficial. I endeavored, for the sake of great certainty, to scrape off one of these spots from the soft palate; I was unable to do so, and the effort occasioned in the patient a violent attack of vomiting. He complained of extreme distress in the posterior part of the nasal fossæ, which attained its height during the action of deglutition. The pulse was 130, large and soft. On account of the great number of cases of eruptive fever which existed at that time, the idea of commencing scarlet fever naturally presented itself to my mind; but, on the one hand, the mother of the patient had died (in 1845,) of an attack of pseudo-membranous angina, and it is well known that this form of angina may be called a family disease. On the other hand, the plastic infiltration of the pharyngeal mucous membrane, and the false membranes on the surface of the tongue, were unmistakable. Diphtheritis then existed, and in a man hereditarily predisposed, there was reason to fear that this affection, arresting the scarlatinous eruption, would pursue its course the same as if it were idiopathic.

"I therefore decided to apply leeches, in order to diminish the inflammation, and to give the bicarbonate of soda, in large and often-repeated doses, to counteract the excess of plasticity of the blood. I prescribed twelve leeches to the submaxillary regions, (six on each side,) and 12 grammes (3 drachms) of bicarbonate of soda, in twelve powders, one to be taken every half hour, in a spoonful of sweetened water.

"This was at nine o'clock in the morning. At one o'clock, the patient had taken 8 grammes (2 drachms) of the bicarbonate. The leeches had drawn a large quantity of blood, which still flowed abundantly, evidently less plastic than in the normal state. As to the throat, the appearance of things was astonishing, and afforded me as much surprise as pleasure. The false membranes on the tongue remained, surrounded by a pultaceous, dirty-grey layer, which also covered the gums, where it was white; but the plastic infiltration of the posterior fauces had completely disappeared; not a trace of it was left. In the space of four hours, a most alarming state of things, capable of inspiring the deepest anxiety, had wholly subsided. Was this owing to the influence of the bicarbonate of soda? Such is my opinion.

"In the evening, red points appearing upon the skin, announced the scarlatinous eruption, which was general and intense, and which had hardly begun to fade before it was followed by a miliary eruption of white, serous vesicles, very close together, on the neck and arms, accompanied by short paroxysms, during which the heart beat violently, as in the *suelle*.

"I return now to the essential point in this communication, the disappearance of the diphtheritis in the throat, under the probable influence of an alkaline salt. In the first place, no conclusion can be established in therapeutics from a single case. Moreover, this instance is not so demonstrative as we could wish, since in my patient the diphtheritic angina of scarlatina is much less grave than the idiopathic variety. But, as I have already observed, there was one circumstance, its hereditary character, which gave to the angina, although scarlatinous, a peculiar gravity. Besides, when we reflect upon so sudden a disappearance of the diphtheritis, after the administration of the bicarbonate of soda, we can hardly fail to

see in it an effect and a cause; and we may ask whether the same effect would not take place in idiopathic diphtheritis.

"I have said that the object of the alkaline salt was to counteract the excess of plasticity in the blood; it might also have another mode of action, a local or direct effect upon the diphtheritis. This did not escape the attention of M. Trousseau, to whom I communicated the case, which so much interested him that he desired to try the alkaline carbonates in the treatment of pseudo-membranous angina. The local effect which I have mentioned is easily understood, since a gramme (15 grains) of bicarbonate of soda, in a teaspoonful of water, is rather hard to swallow, and 'scrapes as it goes down,' according to the expression of the patient." — *Boston Medical and Surgical Journal*, translated from the "*Gazette des Hôpitaux*."

ADULTERATIONS OF QUININE. — The high price at which sulphate of quinine can now be had in its purity, has led to its extensive adulteration. Physicians and the public should be on their guard, and obtain the article from safe and reliable houses, else they may be using arsenic, piperine, or strychnine, for all these are employed for adulterating purposes. That many of the nostrums advertised for the cure of intermittents contain one or more of these poisons does not admit of doubt, and in infants and feeble persons even fatal mischiefs are liable to occur. This warning is called for by the facts and information before us. The phenomena of ague and fever, though sometimes ephemeral, are more frequently only symptomatic of grave congestions, and hence the medical man who pretends to have a specific or cure-all for intermittents, is either a knave or a fool. There is no disease which requires more discrimination and skill in diagnosis and treatment than the protean phases of ague and fever, nor is there any malady which more seriously endangers the future health, when mal-treated. — *American Medical Gazette and Journal of Health*.

TREATMENT OF LEPROSY IN CHINA. — It is generally known that an inveterate and loathsome form of leprosy is endemic in several parts of China, and that the subjects of it are frequently immured for life in hospitals, at the instance of the government, by whom the poor people are treated rather as criminals than as patients. At all events, when the disease is severe, it is generally regarded as incurable. Its treatment has been, in many instances, undertaken by American and British medical missionaries, who have done so much for the good of the Chinese within the last twenty years, but too often with but little success. It is very gratifying, therefore, to find it recorded in the report for 1853-54 of the missionary hospital in the western suburbs of Canton, under the care of Dr. Hobson, that successful trials have recently been made of a new remedy for the cure of the disease. This is the seed of one of the *flacourtiaceæ*, a plant known in Eastern Asia by the name of *charul moogra*. The seeds are coarsely pounded and given internally, in drachm doses, twice a day, for a considerable time. The expressed oil of the seeds is applied occasionally to the affected portions of the skin by friction. Dr. Hobson writes thus of the results of this treatment: "I have seen two cases certainly cured, and several others much benefitted; some are still under treatment. The remedy requires to be steadily persevered with for several months. Saline aperients are administered along with it occasionally. The first appearance of improvement is in the eruption becoming less prominent and red, while whitish scales appear around the margins, and the central parts gradually assume the appearance of healthy skin.

The Chinese call the medicine *tæ fung tsze*. A few use it secretly to cure scrofula as well as leprosy. They believe that it produces a change in the blood; and they use the expressed oil as a local application in cases of chronic ulcer, itch, and psoriasis; sometimes, also, it is given by the mouth as an anthelmintic. — *Edinburgh Medical Journal*, 1855.

MORTALITY OF MEDICAL MEN. — In a memoir read before the Physico-Medical Society of Wurzburg, by Dr. Eschschricht, are contained some remarks on the variation of the average duration of life in the different professions. From the statis-



tics drawn up in Bavaria, and which coincide with what Caspar had already proved, it appears that among the members of the medical profession the highest rate of mortality exists. Three-fourths of the medical men die before the age of 50, and ten-elevenths before 60. Of 1,688 medical men in Bavaria in 1852, four only had passed the age of 80; and of these four none had devoted themselves to the practise of medicine exclusively. The united ages of the oldest ten physicians amounted to 792; while the united ages of the oldest ten in each of the other liberal profession presented the following numbers; Roman Catholic Priests, 878 years; Professors in the schools, 875 years; Protestant ministers, 865; lawyers, 885 years. *Association Medical Journal*, from *Bulletin de Therapeutique*.

**ABORTIVE TREATMENT ON CORYZA.**—M. Yvonneau states that during several years he has always succeeded in arresting idiopathic coryza within twenty-four hours, by the simple expedient of occluding the nostrils. He spreads gold beater's skin with collodion, cutting it into strips, and so applies it as to entirely close the external apertures. The person can go out, and were it not for the alteration of voice, the application would not be noticed by others. The irritated membrane is thus protected from the cold atmosphere, and is kept in contact with a moist, tepid air only.—*Rev. Med.*

**CYANURET OF MERCURY.**—M. Desmarts, of Bordeaux, after a careful comparison of the effects produced by the different preparations of mercury, has come to the conclusion that the cyanuret is superior to all others, especially in syphilis. He believes it at the same time efficacious and innocuous in its action; he never saw it occasion salivation or any intestinal irritation; and often, when all the preparations of the metal had failed to produce benefit, he has seen it restore to health patients whose cases seemed hopeless in the extreme. He has found its use to be efficacious in certain cases where the patients had suffered, for a long period, obscure pains, for which no cause could be discovered. He has employed it with benefit in iritis, and in syphilitic affections of the nose and fauces.—*Dublin Hospital Gazette*.

**JUDICIAL DECISION IN ANTWERP RESPECTING MEDICAL SECRESY.**—The Lower Court of Antwerp has just decided that a medical man who registers a child whose mother he has attended in labor, is not bound to divulge the name of the latter, if he have made a promise to keep the secret. The question and the trial had caused much sensation amongst our Belgian brethren, and we are happy to state that a large number of them joined in a subscription to support Dr. Bessens, who refused to give the name of the mother in a case of the description mentioned above. The privilege thus conceded is honorable to our profession, and would have the tendency, were it generally granted, to diminish the cases of infanticide.—*Lancet*.

**PREVENTION OF ABDOMINAL TYPHUS BY VACCINATION.**—Dr. De Gressot has communicated to the Academy of Medicine some remarks upon the probable consequences of the connection established by some medical men between smallpox and typhus fever. He asks if, admitting the connection to exist, it is not desirable to attempt the prevention of the eruption on the intestinal mucous membrane by vaccination, performed upon some accessible point of its surface, in the same manner as the cutaneous eruption is combatted by vaccination practised on the skin.—*Gazette Med.*

**HYDROPHOBIA IN FRANCE.**—From an inquiry instituted by the French Government, it appears that, in 1852, no less than 48 cases of hydrophobia occurred in France, these being distributed over fourteen departments. Returns are now ordered to be made every year; and one consequence of drawing attention to the subject has been the inundation of the Government with infallible specifics. These have been handed over to the *Académie de Médecine* for examination, and M. Bouchardet, in a recent report, exposes their ridiculous pretensions. He observes that there is not one of these recipes which has not at some former epoch been brought forward, and allowed to fall into deserved oblivion. The authors of these communications must also be edified at learning that the *arcana*, which they flattered themselves they alone possessed, reach the Government by different channels, and are, indeed, traceable to quite the infancy of art.—*Med. Times and Gaz.*, 1855.



A CARD LEFT ON A DOCTOR'S DOOR, ON HIS GOING OUT TO TEA.—We lately saw, in a paper of very limited circulation, sometimes which so aptly illustrate certain passages in a "Doctor's" life, that we asked permission of their writer to republish them in our pages. The piece is framed after a well-known model, as will be at once perceived; it was originally intended for, and was read at, a social meeting of the "Norfolk District Medical Society," in 1853, and would never have been printed but for the urgent solicitation of a friend. Without much liking for parody in general, we endorse this as exceedingly ingenious. The signature of its author, B. E. C., is well-known to the readers of this Journal.—[*Editors of Boston Medical and Surgical Journal.*]

THE night-bell rings an end to sleeping aye;  
The low-laid crowds from labors o'er should be;  
The Doctor outward takes his darksome way,  
And leaves his bed for sickness and—a fee.

Now pale the flickering street-lamps in the night,  
And silence reigns beneath the clouded dome,  
Save where the carriage-wheels, with rumbling might,  
Convey late revellers to their anxious home;—

Save that behind his motley-colored door,  
Some yawning Leech may to himself complain  
Of such as, ringing at the midnight hour,  
Buy physic only by the single grain.

Beneath those shingled roofs, that slated pile,  
Where swells the down in many a tumbled heap,  
Each in his cosy bed, forgetting toil,  
Most other dwellers of the village sleep.

The wheezy call of garlic-chewing churl,  
The servant sputtering through the tin-made tube,  
The maid's shrill summons, or the Hibernian howl,  
May never wake them from the sleep they love.

Of them no more shall dangerous camphene burn,  
(Now prudent housewives Newell's patent use);  
No children wake, in fits, ere morn return,  
Or "tedious case" the wished-for rest refuse.

Perhaps in a neglected cot is laid  
Some head all swollen with St Anthon's fire;  
Hands that the druggist's pestle might have swayed,  
Or stirred electuaries in a serf's attire.

Full many a beau of purest "diddling" mien  
To dark unwindowed cell to lodge repairs;  
Full many a bell is doomed to leer unseen,  
And waste her graces and coquettish airs.

Some village-gossip, that with heartless breast  
With little libels all the town would flood;  
Some mute and hoseless fireman there may rest,  
Some colonel guiltless of a foeman's blood.

The moans of sickening babies to allay,  
The attacks of cramp and colic to subdue,  
To dole out physic all the livelong day,  
And hear one's title in the school-boy's hue.

Is not their fate ; nor their's perchance to atone  
 For fatal lesions that all art defied ;  
 They hear no curses for an ill-set bone,  
 Or gaping wound from sutures loosely tied ;

No scolding tongues an unpaid bill to veil ;  
 Or sighs for ailments of inglorious name ;  
 They dress no bruise from enginery or rail,  
 With cerates mingled at the chemist's flame.

Their chintz, their frocks, soiled by unceasing use,  
 The place of silk and honiton supply ;  
 And many a dingy robe around it strews,  
 That warns the shrewd economist to dye.

For who to woful raggedness a prey  
 His fading, napless raiment e'er resigned,—  
 Left his warm bedding 'fore the break of day,  
 Nor cast one longing, lingering look behind.

For thee, who envious of unbroken sleep,  
 Dost thus so loudly thy complainings din,  
 If now, from some affliction sore and deep,  
 A suffering friend should ask if thou art "in,"

Haply a dusty-headed "help" may say,  
 "Oft have I seen him round the house this morn,  
 "Brushing his clothes in haste to get away—  
 "I'm sure I cannot tell where he has gone ;

"Upon yon couch, now tattered o'er and torn,  
 "Mixing his awful physic, he would get ;  
 "Now fainting, hungry, lean, like one out-worn,  
 "Or wanting sleep, or head-and-ears in debt.

"One hour I missed him—hunting up a bill ;  
 "Gone was the hash, and ne'er a bit of tea ;  
 "A patient came ; nor yet upon the sill,  
 "Nor up the stairs, nor on the couch was he ;—

"The next, to driver's seat, both high and hard,  
 "Of the slow, crowded 'bus, I saw him soar ;  
 "Come here and read (for I can't read) the card  
 "Placed as a sign upon the office door."

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#### THE CARD.

Here dwells, awaiting all the haps of life,  
 A doctor humble and of less conceit ;  
 For lofty station never was his strife,  
 And mammon-folly mars not his retreat.

Few are his wishes, with the world content ;  
 His daily recompense enough, though small ;  
 In early studies all he had he spent ;  
 Now gains in practice oft ('twas all he hoped) a call.

No farther seek him till to-morrow's dawn,  
 Let him, uncalled, a casual feast attend  
 (Where he awhile from troubling cares has gone),  
 The supper of a neighbor and a friend.

NEW ORLEANS

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A Report of the Yellow Fever, at Centreville, in 1855,  
WITH SOME REMARKS ON THE DISEASE, AS IT APPEARED IN THE  
PARISH OF ST. MARY, IN 1853, '54 AND '55.

By WM. B. WOOD, M. D.

Centreville, situated on the Bayou Têche, in the Parish of St. Mary, in the midst of a very densely populated sugar growing region, 5 miles below Franklin, and 12 miles above Pattersonville, contains a population of about 200.

In September, 1853, the yellow fever made its appearance at this place for the first time, as an *epidemic*. Indeed, so far as I am informed on the subject, for the first time in any shape.

An account of that epidemic has already been published in the "Report of the Sanitary Commission of New Orleans, on the epidemic Yellow Fever of 1853," and also in "Dr. Fenner's report to the American Medical Association, on the Yellow Fever and Cholera of 1853."

During that epidemic, about 45 or 50 cases of fever occurred in the village—between the 18th of September and the 18th of November—the period at which the disease had disappeared.

It also prevailed in an epidemic form at Pattersonville, at the same period of time, (an account of which was published by Dr. Grout, of Pattersonville, in 1853), and in other portions of the Parish below this; while the upper portion of the Parish, including the town of Franklin, remained entirely exempt from yellow fever, with the exception of one family in Franklin: a report of which was made by Dr. Lyman, of Franklin, in a letter to Dr. Fenner, and published in New Orleans in January, 1854.

In September, 1854, the yellow fever again appeared in Pattersonville, in an epidemic form, and also at Franklin, and upon the plantation of Judge Baker, 4 miles below Centreville. Of these places, Franklin, and Judge Baker's plantation, were severely scourged, while Centreville, lying midway between, remained entirely healthy during the whole season, escaping the yellow fever altogether, with the exception of two or three cases, contracted in Franklin. Of these cases, one died with *black vomit*, in our midst, and yet the disease failed to spread, or communicate to any one else in the village, notwithstanding many were exposed to it, who had never had yellow fever.

Does this fact favor the doctrine of *contagion*?

But let us follow its history on another year. And this brings us to the period more especially, for which *this report* is intended.

The history of the fever at Centreville, for 1855, presents more than ordinary interest, from the fact, that at *one time* in its progress, it was admitted on all hands to be *genuine* yellow fever, by those who examined the cases, and, at another time, disputed by those who had already conceded the fact.

There was another class also, who, having neither the courage to approach near enough to the village, to examine a single case, nor the capacity to judge correctly of the facts, could, standing off at the safe distance of five miles, in company with the *publisher* of that interesting 8 by 10 sheet, at Franklin, styled "The Planters' Banner!" assert most positively that there had not been a single case of yellow fever at Centreville, and that the disease which prevailed there, in an epidemic form, and pronounced to be yellow fever by six physicians, all of whom had examined the cases, was in fact nothing but "*Dengue*!"

But to the history. In September, 1855, we find the fever again making its appearance, at Pattersonville and Centreville, about the same period of the year, and in exactly the same form that it did in 1853, prevailing to about the same extent in Pattersonville as in 1853, but to double the extent in Centreville with regard to the number of cases, and perhaps a little milder in its type, while the town of Franklin—scourged in 1854—remains entirely free from epidemic yellow fever in 1855.

We hear nothing in 1855, of "barriers thrown across the public road a half mile below the town of Franklin," as in 1853, and an *armed guard* stationed there, to "fence out" the yellow fever, and to keep off the citizens of Centreville and Pattersonville from Franklin, lest some of the *poisonous infection* might attach itself to the clothing of those coming into the



town, and Franklin become again the victim of a yellow fever epidemic! On the contrary, the most constant and intimate intercourse is kept up between the three places, during the whole prevalence of the epidemic, both by the Bayou and the public road, and yet, Franklin remains, as in 1853, uninfected.

Does this state of things favor the doctrine of contagion and importation? Not at all! But say the advocates of this doctrine—and of course the advocates of Quarantine, as this disease failed to communicate itself to our town, and become epidemic—and as there were some 80 or 90 cases of the fever at Centreville, and only *one man died*, it could not have been *yellow fever*, and must have been some other disease! It did well enough for *yellow fever* at Pattersonville, because a good many died! *Symptoms* all right at Centreville—*looked exactly like yellow fever!* Acted exactly like it, in all its stages, but, as only one man could be found in the place possessed of sufficient patriotism to give the disease a fair chance at him, and in the conflict to fall a victim—not only to the disease, but, perhaps, to his own folly—that therefore, it could not have been yellow fever!

But, they must give the disease a *name* at all events. It would not do to call it bilious fever, as that disease never appears in an epidemic form of this kind. It would not do to call it intermittent fever, because the fever was not of an intermittent character. It would not do for scarlet fever, for there was no scarlet appearance of the skin, and sore throat. It would not do to call it “ship fever,” or “Chagres fever,” because the bark “Tivoli” hadn’t been around this fall, to be “overhauled and cleaned,” after lying, *high and dry*, several weeks, entirely empty, on the sand bar at Last Island? It would not do to call it the whooping cough—because of there being *no cough* that could be *heard!* But as “Dengue” was a term not well understood, and might be used to mean almost any thing, it suited the purposes of the “wise men of Franklin,” through their organ, the “Planters’ Banner,” to christen the yellow fever at Centreville in 1855, “*the Dengue.*”

Some of these Franklin people are great on-contagionists and quarantinists. They could see no possible way for the yellow fever to get into Franklin in 1853, and appear in the family of Mrs. Smith alone, as it did, except through the medium of “a box of axes,” or a “paper package containing India rubber coats,” arriving directly from New York in a vessel, by the way of Centreville! And strange to say, the “negro man who carried them” from Centreville to Franklin, and the clerk in the store who *received and opened them* at Franklin, escaped entirely.

Again, in 1855, we find that two persons actually die of yellow fever, with black vomit, in the town of Franklin. Captain Leaky—disease contracted at St. Martinsville—and Mrs. Cooper—(how contracted unknown) and the yellow fever fails to spread, or to communicate itself to a single individual among all the nurses and attendants! And yet we find persons contending every where that yellow fever is a *contagious* disease, and nothing but *quarantine regulations* can prevent its spread!

But let us proceed with our examination into the history and symptoms of the epidemic fever, as it prevailed at Centreville, in September and October, 1855, and determine if we can, whether the physicians who examined the cases, and treated the disease, were right in calling it yellow fever?

During the months of May, June and July, there prevailed in Centreville, and in the surrounding country, the usual amount of intermittent and remittent fever, mild in their type and yielding readily to medical treatment.

We had also, especially in June, July and a part of August, among the negroes on many of the plantations, the flux, which was often troublesome to manage, and sometimes fatal. And the whooping cough prevailed extensively among children the whole season.

The spring and early part of the summer, was unusually dry. But in July, and the early part of August, very large quantities of rain fell, and the rays of the sun were exceedingly hot, and flies and mosquitoes became very numerous. During the month of August there were a few cases of bilious remittent fever, on the plantations around, but scarcely a case of fever of any kind occurred in the village during August, and up to the time of the epidemic on the 8th of September.

On the morning of the 9th of September, I was called to visit Mr. Hine, a merchant of our place, who was taken sick on the evening before, with a slight chill, followed by fever, violent pain in the head, back and eyes; eyes suffused and injected, and very sensitive to light—complained of great pain in the eyeballs—tongue rather clean, and pulse about 110. His fever continued between two and three days, and went off very gradually. On the 9th another case occurred, Mr. Bonta. On the 10th two others, Mrs. Whitworth and George Rogers. And in less than *ten days* there were between 30 and 40 cases, of exactly the same kind of fever, under treatment in the village; and this be it remembered out of a population of only about 200.

Of these four cases, three of them were under my treatment, all of which

I pronounced *yellow fever*. The other case, treated by Dr. Fassitt, was pronounced by him to be yellow fever, *at the time*.

Subsequently, Drs. S. Allen, C. M. Smith, and J. W. Lyman, resident physicians of Franklin, came down to Centreville, and were invited by me to visit and examine some of the cases under treatment at the time; and all of them gave it as their opinion, after so doing, that the disease was *genuine yellow fever*. Dr. Ethan Allen, another physician of our place, was taken sick himself soon after the outbreak of the fever, and saw very few of the first cases, enough, however, to satisfy him, as he told me, that the disease was yellow fever, of a mild type.

Dr. Fassitt, who treated a number of the cases during the epidemic, entertained no doubt of the disease being *genuine yellow fever*, until his patients *refused to die!* and so expressed himself to several persons in and out of the village. This fact was sufficient, perhaps, to create a *doubt in his mind*, when he reflected upon the success which attended his practice in the treatment of the yellow fever of 1853 and 1854. But I am of opinion that if he had pursued the same practice in 1855 that he did in the yellow fever of 1853 and 1854, he would have had no grounds to doubt, at least upon that score. He treated the man (from the third day of his illness), who died at the hotel on the 23d of September, and informed me that a dark looking matter, which ran freely from his nose and mouth some twelve hours after death, was, in his opinion, *genuine black vomit*. This *corpse* I also examined myself, some twelve or fourteen hours after death, and I entertained no doubt, from the appearance presented, and from what I had learned of the history of the case, before death, that this man's disease was yellow fever. And I agreed with Dr. Fassitt, in the opinion expressed, that the dark matter running from his nose and mouth at the time, was *genuine black vomit*. Indeed, I have not the slightest *doubt* on that subject. It was as pure looking black vomit as I ever saw thrown from the stomach of any case of yellow fever.

This man was a German by birth; appeared to have been but a short time in the country, as he could speak but little English; was a carpenter by trade, and had been at work in Centreville but a month or two when taken sick. When attacked with the fever, he refused to allow a physician to be called to treat him, and undertook the management of his own case.

From this circumstance, (and all the cases appearing to be of a manageable type,) I felt anxious to see how a case of the fever would terminate when left to nature and the nurse. And hence I watched the progress of his case from time to time as I passed by the door of his room at the hotel,



while attending to a patient of my own in the next room, with the same disease. And from what I was able to see myself and learn from his nurse, a countryman of his, who attended to him, I found that he was taken sick exactly like a majority of the cases under treatment in town, viz: chill, followed by fever, pain in the head, back and eyes. Eyes injected, and very red; tongue clean and moist at the onset, and looking almost natural. Indeed, I did not find a dry tongue among all the cases I attended during the epidemic. Skin hot, in this case, but easily induced to sweat; pulse from 110 to 120; the fever continuing along from day to day, with no intermission, and very slight remission.

He had, from time to time, hemorrhage from the nose and gums. The pain in the head and back continued, with the fever; and the nurse said he never slept at all.

His treatment, in the beginning, consisted of a dose of rhubarb, followed by castor oil, which acted freely on his bowels, and his discharge appeared consistent. He kept very quiet in bed, drank warm tea from time to time, and sweated freely, for the two or three days that I noticed him, and I thought, at one time, that he would recover.

However, getting no better, I presume, about the third day of his illness, Dr. Fassitt was called in to take charge of the case, (as I was informed,) by Cary, the hotel keeper. After this period I saw little or nothing of him until the evening of the fifth day of his attack, when he was said to have *spasms*, and I walked into his room in company with Dr. Fassitt. He was then struggling with strong spasms, and requiring the exertions of two men to hold him in bed. His face was flushed, and his pulse strong and full. Dr. Fassitt corded his arm and bled him from a free orifice, about a quart. Blood dark, and appeared to coagulate in the bowl. Under the bleeding, he calmed down. I know not if anything else was done for him at the time, as I shortly after left the room.

Dr. F. being called away shortly after, requested me to call back to the patient's room again and see him. I did so in about an hour, and found him dead.

Had this man's case not terminated fatally for twenty-four or forty-eight hours longer, I believe he would have thrown up black vomit before death. As it was, the blood was not sufficiently *dissolved* to enter the stomach through the coats of the mucous membrane. He had, however, as before stated, hemorrhage from the nose and gums, before death, and I am fully satisfied the dark matter running from his mouth and nose, at the time I



saw him, 14 hours after his death, was the genuine black vomit of yellow fever. This was the only case of fever that died at Centreville during the epidemic, out of some 80 or 90 cases, and this was the only case that was not *promptly treated* by a physician, *from the beginning of an attack*.

The fever was uniform in its character, in every case; continuing without any intermission, and very slight remission, for from two to four days; and once off showed no disposition to return again, except in case of a relapse. Indeed, in every case, it was a fever of one paroxysm, lasting about three days, attended in all cases, with more or less pain in the head, back and eyeballs; the pain generally continuing along with the fever, and going off only as the fever subsided. In some cases, there was bilious vomiting in the beginning of the attack. In at least one half of the cases, however mild, there was more or less hemorrhage from the nose and gums; and in many of the more violent cases, there was tenderness over the region of the stomach, with vomiting, belching and hiccup, and in every case there was very great *prostration* of strength following the decline of the fever. The red and injected appearance of the eyes, was generally followed by a dull yellow look of the conjunctiva, and yellowness of the skin, attended many of the worst cases of the disease.

The fever began to abate after the middle of October, and by the 10th of November had disappeared entirely. And in this particular resembled exactly the course of the epidemic of 1853, which lasted about 60 days.

Indeed, I could discover no difference in the symptoms and character of the epidemic of 1855 and that of 1853, at Centreville. Both observed not only the same laws in their progress, but the same geographical limits within the village, extending as far up and down the Bayou, and along the public roads, as in 1853, and no farther.

As in 1853, it refused to extend itself into the surrounding country, several from the country exposed themselves to the disease in the village, contracted the fever, returned home, were nursed and attended by their families during the period of their illness, and yet not a solitary instance occurred, in which the fever was communicated to a single individual member of the family, outside of the village.

This was the case also in the epidemic of 1853. The *disease* could only be contracted within the limits of the infected district, and was not capable of propagating itself from person to person in the country, among those exposed either by contagion or infection.

One argument used by those who contend that the disease could not have been yellow fever in 1855, is that the fever attacked all persons indiscrimi-

nately ; those who had had the yellow fever in 1853, as well as those who had not. *This is not a fact.*

I do not believe a single case occurred in Centreville, of well marked yellow fever, during the epidemic of 1855, in the person of any one, supposed to have had *genuine* yellow fever during the epidemic of 1853. My opportunities of judging correctly on this point, will certainly not be disputed, as I treated nearly all the cases in 1853 in the village, and a large majority of them again in 1855. I know that *some few* had fever during both epidemics. But when we understand that *every case* of fever that occurred during the two epidemics was not yellow fever ; and that a number of cases of *plain intermittent fever*, and a few cases of well marked *remittent bilious fever*, so plain that a nurse could at once detect them—occurred during the epidemic in both years ; then, it is not to be wondered at, that some few persons who had fever in 1853, should have fever again, *of one sort or another*, during the epidemic of 1855 !

In the family of John Rogers, his wife alone had yellow fever in 1853, while in 1855 every other member of his family, himself included, had yellow fever ; his wife nursed them all ; and yet she escapes the fever entirely in 1855.

In the family of Ralph D. Smith, his wife and daughter alone had yellow fever in 1853. In the epidemic of 1855, nearly every other member of his family, himself included, have the fever, and the wife and daughter escape.

Other instances of a similar character could be given, but these are deemed sufficient.

Whole families escaped in 1853, and were nearly all sick during the epidemic of 1855.

Dr. Fassitt *thinks* he had the yellow fever in 1853, and says, he had the *prevailing fever* during the epidemic of 1855. And this, I understand, to be the second ground upon which he predicates his opinion, that the epidemic fever of 1855, at Centreville, was not yellow fever.

It was a very easy matter for him to have been *mistaken in his own case*. Physicians are not always the best judges in their own cases.

And, it is also *possible* for Dr. Fassitt to have had yellow fever *both seasons*.

Persons have undoubtedly had the disease *twice*, if we are to rely upon reports made by physicians, and, I am sure, I am not prepared to say that a person might not have yellow fever, even oftener than twice under some circumstances.

Some expose themselves freely, and never have the disease at all. This has been my own case, although exposed to yellow in 1843, '47, '53, '54, and '55. I have so far escaped the disease entirely.

Entertaining *no fears for my personal safety* during the prevalence of yellow fever as an epidemic, perhaps has contributed more than any thing else, to my entire exemption from the disease, during the several years in which I have been exposed.

The *treatment* adopted at Centreville, during the epidemic of 1855, was very simple and very successful. That which I had used here so successfully in the yellow fever of 1853, was the *general* treatment which was followed in 1855. The disease being mild in its type, and no *panic* arising among the citizens, on the subject of the epidemic, yielded more readily to medical treatment.

A mild mercurial cathartic at the commencement of the fever, followed some hours after, by a moderate dose of castor oil, to evacuate the bowels fully; together with warm orange leaf tea as a drink, and hot mustard foot baths, to induce free perspiration during the continuance of the fever; and perfect quiet in bed under blankets, constituted generally, the *medical treatment*, during the paroxysm of the fever.

In some few cases attended with vomiting at the beginning, a gentle emetic of mustard and common salt, in warm water; or, of ipecac, was given first, to empty the stomach, and to assist in developing the fever. This generally induced free perspiration at the start, and tended somewhat to lessen the pain in the head and back. No nourishment was allowed until after the fever began to subside, or most generally, until after it had gone entirely off, when gruel, chicken water, or beef tea, were allowed in small quantities; and brandy toddy, brandy julep, or champagne wine in ice, were given in such quantities as nature required, and the stomach would bear.

While the fever was subsiding, or after it had gone off, a dose of sulph. morphine was sometimes administered, to quiet nervousness and induce sleep; when this could not be taken safely, owing to any idiosyncrasy in the case, a stiff brandy toddy was sometimes given, with a very good effect.

No change was allowed to be made in the bedding or clothing of the patients, between the period when the fever subsided, and the sixth or seventh day of the attack, and the most perfect quiet enjoined upon them, until after this period. This, as a general thing, constituted the treatment

pursued in a majority of the cases. Some cases, differing from the general run, being more violent in their type, required some modification in their mode of management.

Speaking of the use of brandy toddy in this disease, I will here remark, that it will be found one of the very best remedies we can use, to arrest black vomit, and save the life of the patient, after that alarming and fatal symptom has made its appearance.

In one case which I had in November, 1854, of a lady on Bayou Salé, in this Parish, the wife of a sugar planter, I arrested black vomit, which had continued at intervals for six hours, to be thrown up in small quantities, by the *free* use of brandy toddy alone, and the lady is now living to testify to the fact. And although I have seen several die with black vomit, this was the first case of recovery in an adult that I ever witnessed myself, after that fatal symptom had supervened.

My manner of using the brandy toddy on the occasion, was this: I mixed half a glass of the strong toddy at a time, and then gave the patient two or three swallows of it, after every effort at vomiting.

At first, the toddy was instantly thrown up, and the patient declared to me, that it was too heating to her stomach, and that it made her vomit more. To this, however, I paid no attention, I had tried so often before, almost every remedy that suggested itself in such cases, and failed to do any good; and I had tried the plan of keeping every thing out of the stomach, and yet the case would die, whether the vomiting ceased or not; that in this case, I determined to push the brandy, and give the remedy a fair trial. And after every effort at vomiting, which came on at intervals of about ten or fifteen minutes between, I gave a few swallows of the toddy—strong toddy—for my patient was *fast sinking*, with cold clammy skin, small pulse, and hiccupping at every breath. In about an hours' time the toddy began to stick on the stomach, and the black vomit to be thrown up in smaller quantities, and requiring great effort; and in three hours' time, I had not only arrested all vomiting, but I had obtained a warm healthy glow of the skin, and the pulse had become full and round.

She was kept very quiet, and for the twenty-four hours that followed, she took nothing on her stomach, but small quantities of the brandy toddy, at longer or shorter intervals, as the state of the skin and pulse seemed to indicate. On the second day, I substituted champagne wine, for the brandy toddy, and allowed some arrow root gruel, which was followed by broth, etc. As stated before, the lady recovered, and is now in the enjoyment of excellent health.



A few words as to the origin and spread of yellow fever, and I will close this report. I do not believe yellow fever to be *contagious*. I have seen much of the disease for several years past, and I have been unable to discover a single fact, which could induce a belief in my mind, that the disease is contagious; or, that it can propagate itself from one person to another, as we know that small pox, measles, and other contagious diseases, possess the power to do.

I believe the *poison* which gives rise to yellow fever, to exist in the atmosphere. Is generated under peculiar circumstances, requiring the influence of certain degrees of heat and moisture, added to animal vegetable decomposition. That this power, when generated, is of local origin, and confined to certain circumscribed districts of country, or portions of our country, and all who enter within that infected circle, and breathe the air, are liable to take the disease.

That this poison did originate spontaneously at Centreville, both in 1854 and 1855. And that yellow fever here, owed its origin to *local causes*, and not to importation from any other place. That yellow fever will not always spread from the introduction of a case of the disease into a family or community, is established by the fact, that it failed to spread at Centreville, when introduced in 1854, and also, in Franklin, when introduced in the persons of Capt. Leaky, and Mrs. Cooper, in 1855. Notwithstanding, the disease had prevailed in an epidemic form only the year before, at both places, and cases terminating in black vomit, were introduced in both instances, and the whole community were more or less exposed.

Indeed, a hundred instances could be given of just such facts, of persons dying with yellow fever at different places, and nobody else contracting the disease. In some instances, as was the case in this Parish the past fall, a whole family, consisting of the father, mother, and two children, all died of yellow fever in one house, while on a visit to a relative, and although nursed by the members of the family of the relative, at whose house they were visiting. Not a single case of the fever occurs among all the nurses and attendants!

None of the first cases that occurred at Centreville this fall, had been exposed in the slightest degree to yellow fever, from any other point. It is true, Mr. Hine had returned from New York, about fifteen days before his attack. But he came down the Ohio and Mississippi rivers, and not by New Orleans; and in coming home, never made the least stop at any place where yellow fever was supposed to exist. Of the three other cases taken

sick about the same time, none of them had been out of Centreville to expose themselves. All lived in different parts of the village, and only two of them had been near enough to Mr. Hine, to speak to him since his return from the North.

CENTREVILLE, La., December, 1855.

## Extirpation of a Cancerous Tumor of the Parotid Gland, WITH LIGATURE OF THE COMMON CAROTID ARTERY.

*By M. SCHUPPERT, M. D., of New Orleans.*

Thomas McNuff—aged 24 years—a native of Ireland—for several years past a “hand” on board the steamboat *Lenora*, of Lake Ponchartrain—of good constitution—was never sick until seven years ago, when he discovered a small tumor, size of a bean, on the middle of the right cheek. The tumor was situated immediately under the skin, was moveable, and painless even under pressure. Two years afterwards, he was seized with an attack of fever, and entered the Charity Hospital. In due time he recovered from this attack, and was transferred to a surgical ward. Here, he had his tumor extirpated, it having now reached the size of a pigeon’s egg. The wound healed, and he was discharged from the hospital. For some time, he experienced a sensation of soreness in the cicatrix, and six months after leaving the hospital, he discovered a swelling in the seat of the operation. This tumor continued to enlarge gradually, and two years ago *another* tumor appeared, between the angle of the lower jaw, and the mastoid process of the same side, and the latter soon showed a disposition to out-grow the former.

At the time I first saw the patient, (September last,) the two tumors were occupying nearly the whole of the right side of the face, being separated only by a small fossa, corresponding with the middle of the masseter region. Both tumors were of an uneven appearance, like an aggregation of cysts—some of the lumps being hard, while others were soft and elastic to the touch, as though they contained a fluid. The front tumor extended from the orbicular muscle of the eye, to the lower jaw bone, pressing the nose to the left, and including the upper lip of the mouth, the whole affecting a very singular distortion. The posterior tumor was more of a coni-

cal form, spreading from the zygomatic process of the temporal bone, downwards below the angle of the lower jaw, and including the anterior portion of the ear.

The patient received a blow with a piece of wood, two years before I saw him, on the middle of the anterior tumor, which caused an opening about the size of a twenty-five cent piece, and from this wound, there was discharged a thin yellowish fluid, which continued to flow during the two years. The wound did not cause the tumor to increase, nor was there any consequent alteration. Touching the wound with a probe would cause hemorrhage. Both tumors were quite fixed in their respective situation, though the jaw bone was thought to be intact. Patient never experienced pain, nor was there as yet any impression on the general health to be observed. During the month preceding my first acquaintance with him, his powers of mastication had become diminished, and deglutition was somewhat difficult, so that he had become desirous of having an operation performed. Having placed himself under my care, I advised him to go to the Charity Hospital, in order that I might there operate on, and attend to him.

Considering the location and extent of the disease, there could hardly be any doubt that the auricular and temporal arteries, the transversalis faciei, and the maxillaris externus were to be divided—aye, even the internal maxillary might be involved. The apprehension of serious results from cutting so many important blood vessels, has induced some surgeons to tie the external carotid before operating—others to secure, even the common carotid. But, considering that it is almost impossible to ascertain correctly the extent of such a morbid growth; that in many cases of extirpation of the gland, (it was thought necessary here,) the internal maxillary was not divided, and could be saved; that history furnishes instances where the blood vessels, instead of being enlarged, were nearly obliterated; that the bugbear of blood-shedding is of no importance to the surgeon, who adheres to reason and presence of mind; and lastly, that a ligature of the common carotid, is more dangerous to the vitality of the brain, and is an operation of more importance than extirpation of the parotid gland—I determined to act according to circumstances revealed by the process of operation.

The patient was put under the influence of chloroform. In order to save the effusion of blood, and to gain time, I deemed it preferable to commence the operation on the tumor involving the parotid gland, as only in the extirpation of this tumor, it might become necessary to take up the carotid.

The scalpel was inserted above the growth, and a longitudinal incision made through the skin, covering the tumor down two-thirds of its length, and then turned to either side at an obtuse angle, giving the whole wound the shape of an inverted Y. The flaps being loosened and turned back, the scalpel was gradually introduced around the growth, and where the shape of the tumor required it, a pair of curved scissors was substituted. The tumor was hard and immoveable, and considering the extent to which it was wedged in under the neck of the lower jaw, there could be no doubt, that ligature of the external carotid, at least, had become necessary. The arteries already severed, were so closely imbedded in the surrounding parts that a ligature "en masse" could only be resorted to. To obviate this, and to save time, they were compressed by the fingers. The incision of the tumor having now progressed nearly to the internal maxillary, it was found impossible to avoid cutting this artery; it being, likewise, imbedded in the tumor, and it being impossible to ascertain the depth to which the tumor reached, it was deemed advisable to apply a ligature to the common *carotid*, instead of to the external. Under existing circumstances, an incision was, therefore, made, beginning opposite the middle of the thyroid cartilage, corresponding to the margin of the sterno-mastoid muscle, and one and a half inches in length. The skin, platysma-myoides, and superficial fascia being divided, the mastoid muscle was loosened from the cellular tissue, and the edges of the wound drawn apart by means of blunt and broad hooks, whereupon the bundle of vessels became visible. The common sheath of the vessels was opened by means of two forceps—a method not in exact consonance with the precepts of fashionable surgeons, but which should, nevertheless, be recommended in such critical operations.

The internal jugular vein was found rather inside the carotid, and nearly covering it, besides having some branches, without the severance of which, an approach to the carotid became impracticable. The bleeding caused by this, checked the process of operation for some time. This anomalous state of the internal jugular, which rendered the operation more difficult, was accompanied by a considerable turgescence of the venous plexus, and quickened respiration, by which the recurrent stream of blood was retarded. The mass of blood forced back to the vena cava by the systole of the right auricle, and causing dilatation of the jugular vein, (already in a state of turgescence,) contributed still further to conceal the carotid artery; so that it was only at very short intervals that the approach to this vessel could be



effected. To encourage the patient, under such circumstances, to deeper inspirations, for the purpose of overcoming the above mentioned obstacle, was rendered almost impossible, by his being under the effects of chloroform — the administration of which, will certainly not be abandoned in such cases, when we consider of how little use it is to try to encourage a patient, who, with the knife on his throat, and exceedingly agitated, is by no means inclined to obey the surgeon's call for a succession of deep and regular inspirations. These being overcome, however, and the ligature having been applied to the artery, the extirpation of the whole tumor could be effected.

The greater part of the styloid process became visible in the depth of the cavity. The carotid artery, and as a matter of course, the facial nerve were divided. The hemorrhage which followed the extirpation of the growth, including the whole parotid gland, was, as was to be expected, inconsiderable; and the same may be said of the removal of the tumor on the cheek, with the exception of some bleeding from small arterics, which was stopped by ligature and torsion. In the removal of the latter tumor, a part of the skin, in the middle of which the ulcerous opening existed, had to be cut out by semilunar incisions. The removal likewise, of a portion of the mucous membrane of the cheek, could not be avoided.

The extirpation of both tumors was complete, and no morbid growth was left behind. The wound, after being filled with lint, and the flaps of the skin fastened with adhesive strips, was warmly covered. The patient not being very robust, was, notwithstanding the small loss of blood, considerably exhausted. Some brandy and a dose of morphine at night, were given him. There being no unfavorable symptoms perceptible on the third day, the dressing was removed, and the wound was cleansed with chamomile tea, and again dressed with lint, saturated with vinum opii. The flaps of skin presented slight discoloration, leaving no apprehension, however, of mortification. The patient had slight cough, complaining besides, of a little pain in his throat, and difficulty of deglutition, which symptoms require no explanation.

He was dismissed from the Hospital four weeks after entering it, and after the further expiration of three weeks the wounds had nearly filled by granulation. From this time, however, every effort to bring the wound to the level of the surrounding parts was fruitless. The edges of the skin, as turned inwards, were then pared off, so as to unite the wounds by sutures, and this plan succeeded in effecting a closure so far as the wound over the seat of the parotid gland was concerned. The closing of the wound on the cheek

has still to be effected by means of a plastic operation, there having been necessarily great loss of tissue in the extirpation of this tumor. In paring the edges of this wound, considerable hemorrhage ensued from an artery answering, by its position, to the transversalis faciei. Tortion being made on the vessel, the patient cried out with pain, produced by a nerve coming between the blades of the forceps.

It is further to be remarked, that the patient, ever since he was operated on, has had all the sensibility of nervous action, and has only slight paralysis of the lower eye-lid. Another singular phenomenon is presented in the obstinacy of the healing of the wound where the carotid artery was ligated. The ligature could only be removed on the 19th day, and a fistula formed again after an effort to close the wound by the suture. The surrounding parts were hardened and inflamed to the extent of  $1\frac{1}{2}$  inches in circumference, and the hardness has yielded only in the last day or two to the external application of the extract of digitalis.

The microscopic examination of the tumors shows them to be composed as follows—

1st. Small cells, containing large nuclei and nucleoli, bearing all the characters of the cancer cell. 2d. Fibro-plastic cells found situated between the abnormal growth and the healthy tissue, forming, as it were, a limit to the former. 3d. Cysts of various sizes, containing colloid matter and cancer cells. No free nuclei. The blood vessels presented no lesion. It was impossible to obtain even the smallest particle of nerve tissue for examination. The tumors had been in alcohol for several weeks, which has had some influence in the size and appearance of their elements, though there can be no doubt of their cancerous nature.

The views of Allen Burns and Dessault, about the impossibility of the total extirpation of the parotid gland, have been refuted by repeated facts, it having been successfully performed by Chelius, Dieffenbach, Lisfranc, Kline and Walther. "The saddest facts," says Dieffenbach, "which experience has brought to light, result from section of the facial nerve, as thereby a total paralysis of the corresponding side of the face, and great deformity, is produced; yet it may be avoided in general, by a careful manipulation."

Our patient has not suffered the least deformity of features in consequence of section of the facial nerve, nor is there any paralysis perceptible except that before-mentioned. It is not our intention to controvert here, the experience of such high authority, but in regard to the proposed careful preservation of the nerve, we must give our unqualified dissent. Every

one who knows the difficulty of avoiding this nerve in operating on the cadaver, will look upon this proposal as wholly impracticable.

The celebrated anatomist, Hytle, associates himself with Allen Burns and Dessault, denying the practicability of total extirpation of the parotid gland, on anatomical principles—he being further of opinion, that the performance of this operation is impracticable, without the previous application of the ligature to the external carotid; declaring, besides, the lesion of the internal carotid to be rude surgical practice, on the ground that its separation from the gland by fibrous tissue should be a protection to it. If Hytle would but consider the alterations of every organic tissue coming within the scope of malignant tumors, and did not disdain the experience of pathological anatomy, he would be far from making such assertions, and acquire the conviction that the protection of his fibrous tissues is not to be estimated so highly in the face of such morbid affections. The ramifications of the parotid gland in the normal state, may create a doubt about its complete removal, but “there is something of a hiatus between normal and morbid anatomy.”

We will finally greet as progress in science the improvements in diagnosis, (that of fixing the distinction between innocent and malignant tumors), which has shed so great a lustre on Rokitanski, Paget, &c.; but we are not inclined to accord these improvements that high position generally awarded them in the field of operative surgery. The question of the nature of the *constituent elements* of a tumor will always be of secondary consideration; it being more important to ascertain the influence the tumor exerts on the continuance of human life. If the presence of a tumor endangers life, the practical surgeon will prefer the use of the scalpel to diagnostic subtilties, no matter whether the tumor ranks in the category of malignant growths or not.

In closing this article I must express my great indebtedness to those gentlemen who so kindly assisted me in the operation, and more especially to the indefatigable and intelligent House Surgeon of the Charity Hospital, Dr. S. Choppin.

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## Communicated

By DR. E. D. FENNER.

*Messrs. Editors:*—I beg leave to invite your attention to the following interesting communication, which appeared in the *Medical Examiner*, (of Philadelphia,) for September, 1855. It contains such valuable sugges-

tions in relation to the arrest and prevention of pestilential diseases, that I trust you will not refuse to give it a prominent place in your Journal. I know not who the author, Mr. Stamm, is, but if the facts he gives relative to the origin and disappearance of the plague in Egypt are as *reliable* as they appear to me *reasonable*, they are well worthy the serious consideration of those who at this day seek to discover the origin and to arrest the progress of a similar pestilence, which threatens to produce as wide-spread destruction in our own beloved country as did the far-famed Egyptian scourge in the old world. Yellow fever and plague are nothing but the most malignant forms of *endemic fever*, in their respective regions, and hold the same relations to the various milder types. We have the authority of Baron Larrey for the fact, that among the various pestilential diseases—such as plague, dysentery, etc.—that afflicted the French army during the invasion of Egypt by Napoleon, yellow fever made its appearance with all its characteristic symptoms. The plague certainly originated in Egypt, and from total neglect of all sanitary measures. For centuries it was confined there and to the surrounding region; yet in the course of time it traveled off to distant places, and, during the fifteenth and sixteenth centuries, it devastated Europe as high as the fifty-sixth degree of north latitude. Let him who would seek the cause of its cessation in Europe, compare the accounts we have of the sanitary condition of its large cities at that period, with their present state, and he will be astonished at the result.

Yellow fever is the great *endemic* of our country, and we know from past history that it may, and has prevailed as far north as Boston and Quebec. *Sanitary reform* has almost expelled it from our northern cities; but in the South, where these important measures are so much neglected, the disease still rages from year to year with all its pristine severity. It is but little over half a century since it began to prevail in the lower delta of the Mississippi; but behold what rapid progress it has made. It is evidently enlarging its area from year to year, and it is by no means improbable that in a short time, unless something be done to arrest its progress, it will become one of the common endemic diseases of all the Southern States. But the question is, what can be done to rid us of this evil? Let us look abroad and see what has been effected by sanitary measures. Let us consult the history of the past, both in this country and Europe, and learn wisdom from its instructive pages. New Orleans is the natural *emporium* of the Mississippi Valley. Here this disease has committed its greatest ravages, and *here* should begin the great work of *sanitary reform*. If the plague could be expelled from Cairo, surely we may hope to see



yellow fever expelled from New Orleans. But it can only be done by similar measures. If we had some despot like Mehemed Ali, to *compel* us to save ourselves from untimely destruction, we should, in this respect at least, be better off than in our present state of *popular sovereignty*, where there is no *supreme will and power* to guard and protect the public welfare.

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“THE PLAGUE, ITS ORIGIN AND DISAPPEARANCE—*By Aug. Theod. Stamm.*—It is the object of this paper to give some information of the most destructive of all diseases—a disease which for centuries and centuries, resisting the efforts of the most learned physicians, has produced the greatest misery and many scenes of deepest immorality and degradation. No one has yet explained the total disappearance, for the last ten years, of this scourge; neither has the real place of its origin ever been demonstrated. I do not pretend to know the nature of the specific plague poison; no one has yet recognized its essence. But from my own observations in the native country of the Plague, I hope finally to prove where this poison was year after year generated, and by what means it has been eradicated.

The Plague, *Pestis seu Pestis bubonaria orientalis*, *Typhus pestis seu Typhus pestilentialis*, die Pest, la Peste, Typhus d'Orient, Peste, Pestilenza, Pestilencia, etc., was a disease chiefly at home in Syria, Asia Minor, European Turkey and Egypt, but often spreading over the whole coast of the Mediterranean, and even over the whole of Europe, penetrating far into the interior of Russia, and when once there unchecked by the northern winter of Moscow.

“It is difficult to decide whether the Plague be identical with the epidemics recorded by Moses, David, Homer, Thucydides, Herodotus, Dionysius of Halicarnassus and others, or whether the beginning of this disease may be found in the Justinian Plague, which from 542–594 raged almost without interruption in all parts of the Roman Empire, because in these old writers the symptoms are not described with such accuracy as to arrive at quite certain results.

“Thus much we know for certain, that since the time of the Justinian Plague the disease has repeatedly visited not only large countries, but almost the whole of the old world, and only since this time it seems to be characterized by buboes; and after once having adopted this form it appeared indestructible, raging for more than a thousand years, and in some places never disappearing.

"I do not intend to enter here into a minute description of the Plague, but only mention that it is a fever of the most aggravated kind, chiefly characterized by buboes or inflammation, swelling and suppuration of the lymphatic glands, although, according to the special properties of the disease, many varying symptoms have been observed; thus at the commencement of the Plague, cases where nothing is to be observed but the symptoms of a severe typhus fever, the patient generally dying in the course of a day, or from apoplexy (apoplexia pestilentialis) after a few hours of great prostration, whilst towards the end of an epidemic it assumes often such a mild form that a slight swelling of the lymphatic gland and a mild fever is all that can be observed.

"Those who want more accurate information of the different forms and symptoms may find it in the writings and reports of Thomas Sydenham, Heberden, Diemerbroek, Nathan Hodges, Pestalozzi, Chicoyneau, Beerwinckel, Rothman, Melzer, Patrick Russell, Mead, Chirac, Enrico di Wolmar, Verney, Deidier, Bertrand, Souliers, Clot-Bey, Bulard, Lachèze, Gaetani-Bey and numerous other medical men, many of whom not only treated and observed the disease, but in their heroism and scientific zeal exposed themselves with undaunted courage to the danger of post mortem examinations.

"*The treatment of the Plague.*—The opinion of physicians in regard to the treatment of the Plague, were but too often in contradictory opposition. The antiphlogistic method had a most celebrated defender in Thomas Sydenham; but after more active inquiry it was found to be erroneous. Chicoyneau, as well as Audon, Deidier, Verney and the other physicians at Marseilles, were much opposed to this method. Russell and Enrico di Wolmar did not reject bleeding, but whether they ever derived real advantage from it, remains, at least, very doubtful. Palloni was in favor of calomel; but according to Bulard, calomel is always thrown off without effect, by vomiting or stools. Paris thinks that nothing but cold water should be employed. Savaresi, Bulard, Pugnet had recourse to a slightly stimulating system, and tried ether, alcohol, camphor, ammonia, etc., but success did not recommend these remedies.

"The system of evacuating the stomach and bowels has, if properly administered, frequently proved beneficial; emetics and strong cathartics, given at the beginning of the disease, have been considered the best remedies. The diuretics have been recommended by Sydenham to be employed in combination with the antiphlogistic treatment; and Mead is also a defender of the diuretic method. The invigorating system in which the cor-

dials were used had for some time a great reputation, but from their want of success their use was quite abandoned. Narcotics, too, have been tried; opium, strychnia, lactucarium, and belladonna have been given. Dr. Aubert prescribed Hachisch in the Egyptian Plague of 1833. Frictions with oil and various other treatments have never done very much for the cure of the disease. In the treatment of the buboes and carbuncles, science was likewise at fault, and did not yield successful remedies; the simple surgical method has proved as good as any other. But, however treated, about two-thirds of those attacked have always died. Emetics and cathartics, given at the beginning, cleanliness, cold water and demulcent drinks would probably, if generally employed, have yielded the best results. The poison once generated, is mightier than all the remedies used against it, and coming like a scourge upon careless man, should teach him that the great aim should be prevention rather than the cure of disease.

*“The generating of the Plague.*—Great aridity after copious rain and inundations, winds constantly blowing from the same direction, sudden change of the seasons, scarcity of food, etc., have been found concurrent with the entrance of the Plague, and the variation of those conditions seemed often connected with the course and character of the disease. But how uncertain such indications are, is manifest, if we consider that frequently in the midst of prosperity, and during the most agreeable weather, the Plague attacked populations quite unconscious of the dreadful evil so near them. Such striking contrasts annihilated all theories in reference to its generation, and, at those times, when, during the shorter or longer intervals, the disease was spread over the whole of Europe, the people were lost in the most uncertain conjectures.

“At last the more civilized nations of Europe inclined to the opinion that the East and Egypt were the principal countries of the Plague, as this epidemic was found to be endemic there, even at times when the more cultivated and cleaner parts of Europe were exempt; and as the intense contagious power of this disease had manifested itself most terribly, and cases of Plague frequently occurred in consequence of the intercourse of men and the exchange of merchandise along the infected frontier, it was resolved that, in order to prevent its introduction, the quarantine should be established, and that if the disease should, notwithstanding, be introduced, a *cordon sanitaire* should be drawn around the infected district—measures which were executed with the approbation of the inhabitants, who were in constant dread of an outbreak of the epidemic.

"After this it became evident that the plague-poison came always from the East and Egypt towards the protected frontiers, whilst formerly it wandered through Europe in all directions, longitudinally and latitudinally.

"This is in itself sufficient to contradict the views of ancient and modern physicians, who wish to maintain that the disease is alone dependent upon the condition of the atmosphere and the change of weather and season.

"The real sphere of action for the Plague being in this way confined to the East and Egypt, that is to say to the Turkish Empire, in which no mutual quarantine existed, the controversy sprung up where, in this Empire, the exact spot for generating the disease was to be found.

"The lower part of the Danube, Constantinople, Smyrna. Trebisonde, the capitals of Barbary, Cairo, etc., were distinguished as places where the Plague was chiefly at home, and one of the cities often accused the other of being the generator of the epidemic.

"Egypt, which exhibits so many peculiarities in its natural conditions, had indeed for a long time been looked upon as the native place of the Plague, but even sensible and learned writers opposed this view; thus, Prosper Alpinus says that the Plague is imported into Egypt from Greece, Syria and Barbary, whilst Volney, Enrico di Wolmar, Olivier and others thought that the real seat of this disease was to be found at Constantinople, because there it was never quite extinguished for any length of time.

"The controversy where in the Turkish Empire the Plague was generated became the more confused the more interest the inhabitants of each city and country had in clearing themselves from such a reproach. The disease indeed usually came from Alexandria to Constantinople, still several instances occurred where Alexandria was infested by Constantinople. Yet at last the opinion gained ground that, however favorable to the Plague was the filth of Constantinople and other places, it could there only be considered as an exotic, indigenous to and coming from Egypt. Thus L. Ch. Roche wrote an article in the *Dictionnaire de Médecine et de Chirurgie pratiques*, t. xii. 1834, by which he tries to prove that the Plague is indigenous to the country of the Pharaohs, citing, in his favor, the opinions of Desgenettes, Larrey, Pugnet, Savaresi, (physician to the French-Egyptian army), Pariset and Lagasque, (members of a commission in the year 1828, sent by the French government for the study of the disease), Paris, Fodéré, etc.

"But if, according to the opinion of most physicians, we have to accept Egypt as the country whence the Plague has most frequently extended it-



self; the question again arises if in Egypt the cause at once originated all over the land, or if it spread from certain fixed points. In reference to this question, it becomes evident, after earnest inquiry, that during past epidemics, Cairo and the villages of the delta, near this city, were generally attacked first and suffered most. The reason of this phenomenon may easily be found in the condition and situation of the place.

"Cairo has several thousand inhabitants. Before the watering and sweeping of the streets was introduced by the viceroy Mehemed Ali, they were full of filth. A canal which goes through the city and receives all kinds of refuse was very much neglected; its borders had always been considered as most unhealthy, and most frequented by the Plague. Moreover, Cairo was surrounded with an almost complete circle of hills, 150 to 300 feet in height, and where these ceased, by a projection of the Mokattam mountain. Thus purifying winds were cut off from the city. The disease always appeared after the receding waters of the Nile had left much animal and vegetable matter decaying and producing miasmata under the combined influence of heat and moisture, and after raging several months, disappeared with the Nucta, (a heavy dew), and the scorching rays of the June sun. Nowhere, throughout Egypt, was the generating of such miasmata more favored than in and around Cairo; the refuse of several hundred thousand inhabitants, the neglected canal, and a marsh near the city, exhaled their poisonous gases, which, at once, ripening in this basin surrounded by hills, seized upon thousands of people, whose neglected corpses increased the corruption of the air. In this way, first Cairo and the villages around it were desolated, then other parts of Egypt, the Turkish Empire, and before the introduction of the protective quarantine and *cordons sanitaires*, almost the whole of the old world; the disease traveling in all directions, communicating itself to healthy places. Thus the Plague of Marseilles infected other parts of the province and southern France, but it came, nevertheless, originally, from Egypt to Marseilles.

"*The Disappearance of the Plague.*—Mehemed Ali gave orders to clean the city, and to water and sweep the streets every morning, but the state of health did not materially improve. It had already been remarked by physicians of the army at the time of the French-Egyptian expedition, that the encircled position of the city, combined with other unfavorable circumstances, must be very unhealthy. Advisers of Mehemet Ali repeated the remark, and the viceroy, who was a tyrant, but seldom shrinking from the extent of an enterprise, took the bold resolution of carrying down a large portion of the earthy hills into the lowest fields, which, after having suffi-

ciently elevated, he intended to water artificially and to convert into beautiful gardens. As once the Pharoahs dragged thousands of men to the erection of temples and pyramids, so Mehemed Ali forced thousands of felahs (Egyptian peasants) to execute his plans.

"Many died under the excessive labor, but the ranks were filled by others, and the work itself was always advancing. Thus a long chain of hills was carried down and miasmatic fields converted into charming olive and fruit gardens. And as the work progressed, the health of Cairo improved. Already in 1843, but a few cases of Plague occurred; and the disease, no longer brought from Egypt to other parts of the Turkish Empire, disappeared. At the end of the year 1844, while in Egypt, I could find no Plague, and only a few cases of slight swelling of the lymphatic glands. I was assured the disease would certainly reappear in the beginning of 1845; and going to the southern parts of the country, I expected, at my return to Cairo, to find it; yet since that time there has been no return of it. All this shows, conclusively, that Egypt was the real seat of the pestilence; and that not the whole country, but this encircled spot, at the beginning of the delta, generated this disease. Its disappearance, after the falling of the Nucta and the entrance of the summer-heat, is also easily to be understood, if we observe the natural phenomena. The falling dew purifies the air, and the excessive heat immediately following, dries up all the vapors and all putrid matter, scorching over dead bodies and rapidly converting decaying matter into a mere powder.

"Yet I do not mean to say that this or a similar epidemic might not, under favorable circumstances, find its origin also in other southern countries, chiefly where war is raging, where food is scarce, where swamps abound, or swarms of dying locusts and other impurities poison the air; but I assert, man has it always in his power to prevent the disease. War and uncleanness can be avoided, food can be provided by wise measures taken in time, swamps can be drained, the locusts can be buried in the earth, thus doubling the fertility of the soil. Mehemed Ali has proved what can be done even under the most unfavorable circumstances, by his grand and energetic measures in improving the health of one city; and by thus destroying the germ of this most destructive of all diseases, he has unconsciously saved the lives of millions.

"May the extirpation of the Plague contribute to extend the application of the great principle, that the best remedy for diseases and for all other evils, is their prevention rather than their cure.

## Review — Physiological Chemistry:

*By Professor C. G. LEHMAN.*

Translated from the Second Edition, by Dr. G. E. DAY, of the University of St. Andrews, 2 vols. Philadelphia, Blanchard & Lea, 1855.

There is no department of science, so prolific in change, so necessary to be constantly under review as that which treats of chemistry, and especially that portion of chemistry which professes to investigate the change which take place in the organized world, and particularly in the animal frame. Difficult as it is to understand the varied and manifold transformations that occur in the vegetable world, how much more difficult and hard of comprehension is it, when we seek to unfold the complex metamorphoses of the animal organism, and especially that of man — complicated as it so frequently is by disease. It is for that reason, that any work professing to throw light upon such a subject, is especially welcomed, and when we find an author like Professor Lehman, devoting his energies to such a task, we hail with delight the emanation of his intellect, and wait with impatience the developments of his investigations. Probably, no work upon the subject is so full and comprehensive as the one now under consideration, and none will be found so generally useful to the advanced student and practitioner.

The methodological introduction which introduces the work, is deserving of the most careful perusal, as in it, the author points out many of the shoals which have beset the path of physiological chemists, and the rocks upon which they have split. He says, "the errors into which persons have been led by chemical theories and inquiries, have diverged in three different directions." "In the first place, too little attention has been directed to the laws of a true natural philosophy, whose simplest rules have in many cases, been wholly disregarded; in the next place, the necessary causal connection existing between chemistry and physiology, as well as between histology and pathological anatomy, has too often been entirely neglected; and lastly, too much misconception has arisen, from the assumption that chemistry afforded a satisfactory solution to many questions, which [it is either wholly incompetent to answer, or which must at all events, remain undecided in the present state of our knowledge." This last sentence should be engraven on the mind of every chemical inquirer. It has of late been too much the habit of physiological chemists, to attempt the solution of every difficulty by adding or abstracting atoms; it is so easy

to say, that by taking away an atom of water here, or by adding it there, such a change is produced, and such a result effected; and it is time we should be brought to the consideration, that our knowledge is limited, and that there are some things which we do not yet know; every one supposes that he understands fully the theory of digestion, and yet, one man will say, it is effected by lactic acid — another, by muriatic, and so on, forgetting that the stomach is a living organism, and not a crucible. The celebrated Abernethy, in one of his lectures, once said to his pupils, “gentlemen, some persons will tell you the stomach is a fermenting vat, others, that it is a chemical laboratory, but I tell you, it is a stomach, and nothing but a stomach.” If we would only occasionally consider that a man is a man, and not a laboratory, and occasionally think that there is such a power as a vital force, which we can neither make nor understand, we should be much guided and assisted in our inquiries into the wonderful changes that take place within us. Truly does Lehman observe, that “chemical equations having no other foundation than the presumed infallibility of empirical formulæ, must cause us to deviate from the path of physical inquiry, and involve us in a chaos of the most untenable delusions.”

Commencing with the organic substrata of the animal organism, the author takes first into consideration, the non-nitrogenous acids, which he divides into groups, of which, there are seven, viz: the butyric acid group, the succinic, benzoic, lactic, solid fatty acids, oily fatty acids, and the resinous acids.

The first group, when not combined with bases, are characterized by containing four atoms of oxygen, and a multiple of a carbo-hydrogen polymeric with olefiant gas; in their combination with bases, they lose one atom of water, so that the resulting salt, contains an acid in which three atoms of oxygen are combined, with a carbo-hydrogen, whose hydrogen is always too little by one equiv., exactly to produce olefiant gas ( $C^2 H^2$ ), with the carbon.

Dr. Lehman simplifies the consideration of these bodies, by adopting the view first promulgated by Kolbe, viz: that all these bodies are radicals combined with oxalic acid, not as oxalates, but that the oxalic acid is so associated with them, as not to affect their saturating capacity, and on decomposing them, they are converted into oxalic acid, plus the base to which it had been attached. Thus, cyanogen with water, is decomposed into oxalic acid and ammonia. Thus, acetic acid which consists of  $C^4 H^3 O^3$ , may be broken up into  $C^2 H^3$ ,  $C^2 O^3$ , or methyl, plus oxalic acid, and so on, for



all the others. Thus, formic acid yields hydrogen and oxalic acid, and as this mode of considering the composition of these complex bodies, will much facilitate our comprehension of the changes which take place in our bodies, Dr. Lehman has rendered a service by so arranging them. According to this view, oxalic acid being one of the most important elements of these products, is the one first considered. Dr. Lehman coincides with Dr. Bird, in considering the dumb-bell crystals occasionally found in the urine, to be oxalate of lime, and considers they are produced by an arrangement of minute acicular crystals, presenting a physical structure, resembling that of spherical crystals of carbonate of lime. According to the experiments of Dr. Fricke, of Baltimore, these crystals are not oxalate of lime at all, he says, "we do know there is no lime entering into the composition of these bodies, they are rarely present when the urine is voided, and are probably due to the disintegration of uric acid." (Fricke on renal disease.)

With regard to the physiology of oxalic acid, although common to many vegetables, it is remarkably rare in animal organisms, and its presence is almost invariably due to diseased action. For the detection of oxalate of lime in the urine when it exists in but small quantities, Dr. Lehman, suggests the following admirable mode, and which will even detect it in normal urine; let the urine be frozen, in this process a great portion of the water crystalizes in a comparatively pure state, and after its removal, we obtain a concentrated saline solution in which microscopic crystals of oxalate of lime may be discovered. Oxalate of lime is held in solution by filtered urine, and does not separate until the latter has been kept for some little time; may we not suppose that it only falls then in consequence of the liberation of a small quantity of ammonia?

Oxalate of lime is found in the urine of those who partake largely of vegetables containing the salt, such as sorrel, tomatoes, rhubarb, and the like. We have found many persons, ourselves among the number, who cannot chew the officinal turkey rhubarb without suffering from pains in the loins, and we have frequently detected in our own urine oxalate of lime, when we have examined it under such circumstances after having taken rhubarb as a cathartic.

Dr. Lehman disbelieves that oxalate of lime introduced into the stomach finds its way into the urine. We cannot reconcile this statement with the facts of the preceding paragraph, as in those instances there can be no question that the oxalate was introduced with the food, and passed undecomposed into the urine. He also does not find the deposit so frequent, as

stated by Golding Bird, Rees, and others, this may be due to the fact of Germans living on a diet differing from that of England—their food being less stimulating—their use of diluents being greater, and probably also, from their less general excitability. We have found both in England and in America, these deposits remarkably frequent, and rarely see a case of dyspepsia uncomplicated with them.

The pathological origin of oxalate of lime is highly interesting, and Dr. Lehman attributes it in many instances, to the use of food containing alkalis in combination with vegetable acids. In other cases “the increase of the oxalate is due to disturbance of the respiratory process.” Thus, it may be easily understood, why, after the use of drinks, rich in carbonic acid, of alkaline bicarbonates, or vegetable salts, oxalic acid is increased in the urine; the superfluous carbonic acid which has entered the blood, or been generated there from the salts of organic acids, must obstruct the absorption of oxygen, and the perfect oxydation of certain substances in the blood. Hence, also, the amount of oxalate of lime has been found to be increased by the partially impeded exchange of oxygen and carbonic acid in the lungs, consequent on emphysema, pulmonary compression, etc.” It is possible that in these cases, the kidneys act vicariously for the lungs, and remove under the form of oxalic acid the carbon which the lungs should have converted into carbonic acid. The nervous system, also, according to Dr. L., has a great influence on the oxidation of the blood, and on the consequent production of oxalate of lime. We have also seen in a former page, that cyanogen is decomposed into oxalic acid and ammonia; now the elements of urea are also convertible into the cyanate of ammonia with water, and it is exceedingly probable that an excess of urea may be one of the sources whence oxalic acid is derived. We have rarely seen a well marked case of oxaluria, without at the same time finding an excess of urea in the urine, the specific gravity from this cause, often being as high as 1.035. In illustration of this view, Golding Bird, says, that where copious deposits of oxalate of lime have existed for some time, and have resisted all treatment, the persevering use of colchicum, has caused them to be replaced by the uric acid, or urate of ammonia. We have been thus minute in our notice of Dr. Lehman’s views respecting oxalic deposits, from a conviction of the importance of the subject, and from the frequency of the diathesis to which he refers. It is only of late years, that the importance of this salt in producing, or being one of the products of dyspepsia, has been recognized, and we wish to direct the special attention of all practical physicians to the subject.

The other acids of this group presents nothing of importance, their characteristics are very lucidly stated, and are worthy of perusal. With regard to acetic acid, Dr. Lehman has found it in the matters vomited from the stomach, where only vegetables and meat had been taken, and states, that spirit of wine is converted in the stomach into acetic acid, during normal digestion. Butyric acid has been discovered with certainty in the sweat, under Lehman's personal superintendence, but its quantity is less than that of acetic or formic acid, it is not, as he formerly believed, a mere product of decomposition, but occurs in a free state in the sweat of the genitals, axilla, and feet.

Taking into consideration the great importance of lactic acid in the economy, Dr. Lehman has made it the representative of a special group, and states, that he now without doubt has succeeded in obtaining it from the gastric juice; on collecting the gastric juice from fourteen dogs, fed on horse flesh about eight hours before death, he obtained free lactic acid in considerable quantity. He mentions a very important and highly interesting fact, viz: the universal appearance of lactic acid in the saliva of diabetic patients. He states, "in all the cases of diabetes mellitus, which I have observed the saliva has had an acid re-action. Associated with this symptom, and with intense thirst, we sometimes find a copious secretion of saliva, which we have thus a good opportunity of analyzing; as the saliva of such patients, sometimes contains sugar, I took care it should flow directly from the mouth into alcohol, so as to avoid any possible formation of lactic acid from the sugar. The zinc salt which was obtained, showed very distinctly the crystalline form of the lactate." The acid re-action of the small intestines, depends on the presence of lactic acid. This is strongest at the duodenum, and diminishes towards the ilium. In exudations, especially after puerperal fever, and in a case of empyema, lactic acid was found in considerable quantity, also in the juices of the muscles and of the spleen, and in leucoeythæmia. With regard to the presence of lactic acid in the urine, Dr. Lehman states as follows: In all cases where the supply of lactates to the blood is very great, whether this depends on an excess of acid being formed in the muscles; or on the use of a diet tending to produce it; or, on an imperfect process of oxidation in the blood, lactic acid may be detected in the blood. Dr. L., imagines that lactic, like oxalic acid, may be due to the imperfect respiratory action, the kidneys excreting these acids, vicariously for the carbonic acid of the lung, and states, that in almost every case where oxalate of lime exists in large quantity in the urine, lactic acid will be found. Respecting the use of lactic acid, our

author considers it as belonging to the class of combustible foods, and says, "we know of no substitute which could better act on the blood as food for the respiration, than the alkaline lactates, which undergo very rapid combustion in the blood, and are thus converted into a carbonated alkali; in a word, nothing can be a better supporter of animal heat, than the alkaline lactates. It will be recollected, that in an article we wrote in this journal, nearly twelve months back, recommending the use of glycerine in phthisis, on the ground of its forming a large supply of readily prepared combustible matter to the blood, we adverted to some views of Dr. Turnbull, with regard to the use of lactic acid, and stated that glycerine might be considered lactic acid, plus two equivalents of hydrogen. It is interesting in confirmation of the view we then took, to find it supported by so high an authority as Dr. Lehman, for in the concluding paragraph of this chapter, he states, "Finally, after the discovery by Redtenbacher, that glycerine is convertible into metacetic acid, ( $C^6 H^5 O^3$  plus  $H O$ ), there seems to be something attractive in the hypothesis, that glycerine, which in the metamorphosis of the fats, obviously undergoes an independent change, is converted into lactic acid, ( $C^6 H^5 O^5 H O$ ), which is allied to metacetic acid."

Want of space compels us to pass over the highly interesting chapters on the fatty and resinous acids, which are valuable, not only in a scientific, but in a practical point of view. Treating of urea, the author points out a simple and ingenious process by which this alkaloid may be detected in very minute quantities, viz: by separating all albumen, and then digesting the filtered fluid with cold alcohol — rapidly evaporating, and then adding a drop of nitric acid to a drop of the mother liquor, under the field of the microscope. The experiments of Dr. L., on the excretion of urea, are highly important. He proves that it is extremely dependant on the nature of the food taken, on a purely animal diet, two-fifths more of urea is excreted than on a mixed diet, while on a mixed diet, there is about one-third more than on a purely vegetable diet; while on a non-nitrogenous diet, the amount of urea was less than half the quantity excreted during an ordinary mixed diet. We may notice one remarkable circumstance, and that is, that after prolonged absence from all food, there is as much urea passed, as after the use of non-nitrogenous food. Lassigne found urea in the urine of a madman, who had taken no food for fourteen days. In typhoid patients, it is also passed in considerable quantity. Strong exercise also, causes a considerable increase in the secretion.



We think Dr. Lehman is rather unjust in his sweeping condemnation of those diatheses which he says, English physicians have endeavored to establish on certain urinary analyses, and especially does he protest against the urica diathesis—for, asks he, “how does this indicate a morbid process.” We might reply, by asking, how does the presence of sugar in the urine, indicate a morbid process? Surely, if he will admit such a disease as diabetes, he may allow of the presence of such an one as azoturia. If the one is characterized by the presence of the carbonaceous elements of the organism, is not the other equally dependent upon the excretion of an excess of nitrogenized material. He says, “the nature of this or that disease does not depend upon an increased excretion of urea, which is only a consequence of another process.” We certainly do not thoroughly understand the following sentence: “The urea is possibly only excreted, in increased quantity, when material for its formation is sufficiently supplied; now if poly-phagia be not combined with this urica diathesis, the source of the urea must be sought in the waste of the nitrogenous tissues; this is based upon the tendency of the tissues to be converted into urea, but depends on other processes, which accompany many morbid processes.” This is as much as saying that if urea is not produced from one thing it is from another, if the tissues are not in fault, something else must be in fault. No physician yet supposed the presence of sugar in the urine constituted the disease, nor that the presence of pus, in the expectoration of a phthisical patient, was the cause of consumption. We consider them only as symptoms, evincing some organic or functional lesion, so acting on the tissues as either to interfere with their proper nutrition, or ensure their premature destruction. Dr. Lehman himself has stated, that where no food is taken urea exists in the urine. Now if we suppose such a state of things to exist, that where food is taken, its nitrogenized particles shall not be assimilated, urea will still be found in the urine, but will be derived from the tissues, or the metamorphoses of nitrogenized tissues may be too rapid for their repair, and in this case we should have urea in excess in the urine, possibly in this case derived both from the food and the tissues. We have, therefore, the two conditions mentioned by Golding Bird, viz:

*First*—Supply of nitrogenized food not being in excess, but the digestive functions unable to assimilate it, as in dyspepsia, and

*Secondly*—Waste of tissue more rapid than the supply of nitrogenized nourishment, as in fevers, phthisis, etc.—added to which we may have that form in which the whole of the nitrogenized material of the food is excreted as

urea, which together with that produced by the metamorphosis of tissue will be found in the urine, constituting true azoturia, a perfect analogue to though a more curable disease than diabetes mellitus. Even in starvation a train of symptoms occurs precisely analogous to what we find in many exhausting diseases, as phthisis, etc., in which the urine is loaded with urea. In many instances, says Dr. Carpenter, "the body exhales a peculiar foetor, and the skin is covered with a brownish, dirty-looking and offensive secretion." This may be due to the presence of urea or of some of its salts on the skin. We know that in the scaly exudation of lepra, urea has been found, and in the perspiration in some other forms of disease. What we wish to contend for is, that there is a disease, depending upon a certain cause whose characteristic symptom is the presence of an excess of urea in the urine, in consequence of which the body wastes, and the vital powers become impaired. We are sorry to differ from such a distinguished authority as Dr. Lehman, but our experience in this instance is totally opposed to his. We shall return to his highly interesting and most instructive work in the next number of the Journal.

I. L. CRAWCOUR, M. D.

[*To be Continued.*]

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## Editorial and Miscellaneous.

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### HEALTH OF OUR CITY.

During the past two months, whilst disease and death have been steadily doing their sad work in some of the principal towns on the Southern Mississippi, our hitherto fated city, has been enjoying a state of health not equalled in the memory of "the oldest inhabitant." The month of November, 1855, will long be remembered by our inhabitants as the healthiest in the history of the city. Notwithstanding the immense influx of strangers, the mortality reached the low point of sixty-four for the 3d week of the month. For the week ending December 12th, we find a considerable increase — the mortality having reached as high as 103 — but the following week, ending on the 9th, shows a marked decrease, there being only eighty-eight deaths; and since that time, the health of the city has improved until we have again (December 23d,) reached the low point of seventy-seven for the week.

Thus far we have had no cholera, and the last death from yellow fever is reported in the week ending December 16th.

*List of Mortality for Four Weeks, ending Dec. 29, 1855.*

	1st w <sup>k</sup> .	2d w <sup>k</sup> .	3d w <sup>k</sup> .	4th w <sup>k</sup> .	Total.
Total number of Deaths,.....	106	88	80	77	351
Males, .....	54	48	46	40	188
Females, .....	40	30	28	34	132
Sex not stated,.....	12	10	6	3	31
Whites, .....	89	65	69	60	283
Blacks,.....	9	8	6	8	31
Mulattos, .....	8	5	5	5	23
Color not stated, .....	0	10	0	4	14
Native Americans,.....	20	31	14	26	91
Northern States,.....	0	4	3	6	13
Western States,.....	4	2	2	1	9
Southern States, .....	16	25	9	19	69
Foreigners, .....	40	26	32	20	118
English, .....	1	2	0	1	4
Irish,.....	18	6	17	5	46
French, .....	2	9	2	2	15
Germans,.....	10	6	5	9	30
Place of Birth not stated,.....	46	31	34	31	142
Age not stated,.....	5	11	7	13	36
Under one month old,.....	8	11	6	4	29
From one to five years,.....	23	17	12	20	72
From five to ten years,.....	0	4	3	0	7
From ten to twenty years,.....	2	6	2	2	12
From twenty to thirty years,.....	29	18	14	11	72
From thirty to forty years,.....	14	5	6	7	32
From forty to fifty years,.....	11	7	12	13	43
From fifty to sixty years,.....	3	4	6	4	17
From sixty to seventy years,.....	5	3	10	1	19
From seventy to eighty years,.....	5	0	0	1	6
From eighty to ninety years,.....	1	2	2	0	5
Over ninety years,.....	0	0	0	1	1
Yellow Fever,.....	3	2	1	0	6
Other Diseases, .....	76	76	71	63	286
Typhoid Fever,.....	3	1	2	3	9
Cholera, .....	3	1	2	1	7
Intemperance, .....	0	0	0	0	0
Accidental, .....	3	0	0	2	5
Still-Born, .....	16	6	3	6	31
Diseases not stated,.....	2	2	1	2	7

CHARITY HOSPITAL REPORT — For the month of December, 1855.

Admitted, - - - - -	786
Discharged, - - - - -	647
Died, - - - - -	85
Remaining, - - - - -	552
Births, — Males, 2; Females, 5. Total, 7.	

RUPTURE OF THE UTERUS. — *Recovery.* — In the October number of the American Journal of the Medical Sciences, is reported “A case of Rupture of the Uterus, and Recovery,” by Wm. W. Duvall, M. D., of Maryland. The case was one of shoulder presentation, in which delivery was effected by the feet. The placenta was thrown off, but says Dr. D., “the hemorrhage not ceasing or abating, so far as to render the patient’s condition one of safety, it was thought advisable to introduce the hand to provoke contraction, and so doing, I perceived a trans-

verse rent in the walls of the uterus, about three inches above the cervix anteriorly, through which I could easily pass my index, middle and ring finger. The patient recovered readily, and was enjoying good health at the time Dr. D. wrote, having lived *absque marito*.

Instances of recovery after rupture of the uterus are very rare. Amongst the most extraordinary on record, are two cases mentioned by Rigby, in his "System of Midwifery." In each instance "the whole os uteri separated from the uterus during labor." He also mentions the fact of gastrotomy having been twice successfully performed on the same woman, the uterus having in each instance, been so extensively lacerated, as to admit of the passage of the fœtus into the abdominal cavity.

We never treat of the subject of rupture of the uterus without being at once reminded of the beautiful theory of Tyler Smith, as to one of the causes of this terrible accident. He says, "Is it not probable, also, that violent and prolonged muscular action, whether of the heart or of the uterus, favors rupture by softening the muscular structure, becoming thus a predisposing, as well as an actual cause of laceration? It is well known, that in hunted animals, the muscles are found preternaturally soft."

The analogy, when properly considered, is good, and we have always thought that many cases of rupture of the uterus, might readily be prevented by skillful delivery of the child, instead of relying *too* long on the efforts of nature to expel it. True, obstetric authors agree that in the majority of faithfully reported cases of rupture during labor, the rupture occurs at the moment of, or during artificial delivery; but we must not here, as is too often the case, mistake coincidence for cause and effect. No doubt, the irritation produced by the presence of the hand or instrument, is sometimes the *immediate* cause of rupture; but if you will recollect that in the vast majority of cases, the accoucheur only resorts to his hands or instruments for the purpose of assisting the *failing* powers of the muscles, we can readily see the more important, even though more remote cause of the accident. We are no advocates of the indiscriminate application of art in cases of labor, but once the diagnosis of a malpresentation, such as will necessarily require interference is made — let it be three or thirty hours after the accession of labor, we can see no reason for delaying the necessary operation one moment after the parts are in proper condition for its performance. Once an obstetrician has made up his mind that an operation is both absolutely required and feasible, delay is his worst enemy, for it only tends to make more complicated an already complicated case. It is simply ridiculous to speak of the *necessity* for an obsteric operation, and then withhold the hand or instrument for six, twelve, or twenty hours, in the *hope* that nature may relieve herself.

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IODIDE OF QUININE.—M. Pauva, a Neapolitan chemist, proposes the Iodide of Quinine in those cases of intermittent fever complicated with scrofulous affection. Dr. G. Mafredonia, of Naples, has made use of the remedy in such cases with marked success. He uses it in doses from four to eight grains daily.



**LONGEVITY IN FRANCE.**—It is estimated that in France one-sixth of those born, die at the end of the first year, one-fifth at the end of the second, one-third at the end of the fourteenth year; at the end of the fortieth year one-half, at the end of sixty-ninth three-fourths, at the end of seventy-second four-fifths, and at the end of the seventy-fifth year five-sixths.

Anterior to the year 1789, it was estimated that of one hundred persons only fifty reached their twentieth year. Since that time length of life has much increased, and Bienaymé showed that from 1833 to 1844, sixty, instead of only fifty, reached their twentieth year.

Demontferrand states that in one hundred individuals only seven reach the eightieth year; two the eighty-fifth, and one the eighty-ninth—that in one million of persons there were six hundred and forty nonagenarians. According to Mathieu, there are in the same number only four hundred and forty-nine, and of these, nine reached their ninety-seventh year, and only four their ninety-ninth year.

According to Duvillard and Demontferrand, the proportion of centenarians is two in ten thousand. In some countries the proportion is greater; thus in Carlile, Scotland, Milne found nine centenarians in one thousand individuals. In Paris, on an average, one centenarian dies yearly.

Benoiston de Chateauneux having examined the duration of life of fifteen million individuals, found that forty-four persons in every hundred reached the age of thirty—twenty-five reached sixty—fifteen reached seventy—and four reached eighty years.

At the present time the average duration of life in France is estimated at thirty-nine years and eight months. Twenty years ago Bienaymé set it down at thirty-six years, while Demontferrand estimated it at thirty-three years. In 1817, it was thirty-one years. Villermé gives the average at twenty-six years during the seventeenth and seventeen years in the fourteenth century.

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**CANCER AND THE MICROSCOPE.**—It is important at the present time to record all published opinions on the subject of the diagnosis of Cancer through the means of the microscopic test. In the Medical Examiner for December, 1855, we find a notice of a monograph on "The Diagnosis of Surgical Cancer" (Liston Prize Essay for 1854), by John Zachariah Laurence, Surgeon to the Northern Dispensaries, late House Surgeon to University College Hospital. The "concluding estimate" of the author is as follows:

1. That this form of cell is occasionally seen in growths manifestly innocent.
2. That, *vice versa* (what is, however, less frequent), tumors anatomically innocent prove clinically malignant—that "the cancer-cell is not the *sine qua non* character of cancer."
4. That the inferences drawn from the microscopic examination are not to be deduced from a few isolated cells that may have happened to strike the eye, but rather from the character of all the cells, and of the field of view generally.
5. That the result afforded by the microscope must take an important, but not an exclusive and overbalancing position in the series of data, which are to serve us as the premises for our conclusions."

MERCURIAL STOMITITIS.—M. Herpin, of Geneva, praises highly the chlorate of potassa, as a specific in mercurial phylism. He has used it for three years in every case which has come under his attendance, and thus far with remarkable success. He gives it in doses of from ten to twenty grains, repeated every third or fourth hour, in any vehicle calculated to disguise the taste. This treatment is of course more successful when begun soon after the inception of the disease; three or four days generally suffice for the cure of the unpleasant symptoms.

M. Blache has made several trials of the remedy in the *Hôpital des Enfants*, Paris, and has been so successful that he gives mercury more boldly in those cases where its administration is indicated, now that there exists an almost certain cure for its unpleasant effects.

Should the observation of other physicians coincide with those of the above gentlemen, it would be interesting to study the effects of the chlorate of potassa in chronic mercurial affections, such as tremors and paralysis, so frequently seen in those to whom mercury has been injudiciously administered.

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STRAWBERRY LEAVES AS A SUBSTITUTE FOR TEA.—M. Kletzinsky, of Vienna, has lately made a report upon the use of the leaves of the wild strawberry (*fragaria vesca*) as a substitute for tea. When gathered soon after the ripening of the fruit, an infusion of the leaves is a most agreeable dietetic drink. The leaves may be either dried in the sun or in heated pans; the infusion from the leaves thus prepared is greenish, slightly astringent, and somewhat similar to that obtained from the China plant. The infusion is miscible with milk without coagulation, possesses the same diaphoretic and diuretic properties as tea, and is slightly excitant.

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#### NOTICE OF BOOKS AND PAMPHLETS RECEIVED.

“EPIDEMICS AND QUARANTINE: a Lecture, introductory to the winter course, in the New York Medical College, for the session of 1855-6.—By Horace Green, M. D., L. L. D. etc.”

We are indebted to the author for a copy of the above. It is one of the very few “introductory lectures” which pay for the perusal. On such an occasion of course the author was obliged to treat of his subject very generally; yet the lecture contains some useful hints, which, although not original, are rather forcibly expressed, and are of value to all of his audience who note them, as well as to all who will read the pamphlet.

We have received from the publishers, Messrs. Blanchard & Lea, through J. B. Steel & Co., of this city, a new work entitled “An introduction to Practical Pharmacy,” by Edward Parish, member of the Philadelphia College of Pharmacy, etc. The work is designed as a text book for the student of medicine and pharmacy, and as a guide to the physician and pharmacist.

The work is divided into five parts. The first treats of the furniture and implements necessary to the dispensing office; of weights and measures; of specific gravity and the pharmacopœia. Part second gives directions for the collection

and dessication of plants, the manner of making infusions, tinctures, extracts, electuaries, pastes, lozenges, etc., and describes the apparatus requisite for distillation and the process. Parts third and fourth on the pharmacy of plants, their products, etc., and on inorganic pharmaceutical preparations, and part fifth, on extemporaneous pharmacy. The book has an excellent index and table of contents is admirably illustrated by numerous well executed wood cuts, and will be a valuable addition to the library of the practising physician, as well as to that of the student of pharmacy and medicine, and the dispensing apothecary.

We are also indebted to Messrs. J. B. Steel & Co. for a small work, entitled "Healthy Skin, a popular Treatise on the Skin and Hair, their preservation: by Erasmus Wilson, F. R. S. The author's name is well known. The work comprises a short but complete treatise on the above interesting subjects, and is published in capital style by Messrs. Blanchard and Lea. It is illustrated by numerous wood cuts, illustrative of the parts of which it treats, and should find a place in the library of every medical student.

We have received from the publishers, Messrs. Lippincott & Co., through Thos. L. White, Bookseller, Canal st., Simpson's Obstetric Works, edited by Priestly and Storer, and Nelaton's Clinical Lectures on Surgery, from notes taken by Washington L. Atlee, M. D. We shall review the above works in the ensuing number of the journal.

We have received "Elm Fents for the Dilatation of the Curvix Uteri: by Horatio Storer, M. D., etc."

Also—"Transactions of the New Hampshire Medical Society. 1855."

QUACK EXCHANGES.—Again we are called on to decline the favors of those fungi on society—commonly, and more properly, termed *quacks*. Yesterday our box at the Postoffice was encumbered with the "*Eclectic Medical Journal*;" and this morning we find No. 4 of a delectable sheet entitled "*Hunter's Medical Specialist, etc.*"—the latter edited, we suppose, by an individual of consumptive and National Intelligencer notoriety. No doubt these editors expect in return, not only our Journal, but a "puff advertisement;" if the former, we must decline, but the latter we grant them most freely, and hope they will, in accepting this one, trouble us no more.

## Excerpta.

CASE OF POISONING WITH ARSENIC, ATTENDED WITH REMARKABLE DELAY IN THE APPEARANCE OF THE SYMPTOMS.—*By Edward Hartshorne, M. D.*—The following brief sketch of a case of self-inflicted poisoning terminating favorably, is worthy of note, on account of the very unusually long interval that elapsed between the swallowing of at least one drachm, if not two, of arsenious acid, and the appearance of the first decided symptoms of its poisonous action.

I am indebted to the attending physician Dr. Hershey, and to Dr. Griffith, also in attendance after the first day, as well as to the patient herself and to the inmates of the room adjoining hers, for the account of the first four days of her case.

From this it appears that she was a strong, healthy woman, about twenty-two years of age, of a rather excitable nervous temperament, and headstrong impulsive disposition; that she had been subjected for some time previous to great mental agitation, through the threatened withdrawal of an acknowledged admirer, and that under this excitement she had suffered occasional attacks of hysterical spasms. According to her own representation, she had eaten and drunk very little throughout the week; and on Thursday, the twenty-ninth of March, 1855, she took no food or drink except one cup of coffee at breakfast time. She spent the day in a state of desperation, most of the time on foot, first in seeking an interview with the delinquent lover, and then in procuring from an apothecary, at a long distance from her dwelling place, a paper of arsenic. Her absence from home all day, and her entire abstinence while at home, were testified to by the other inmates of the house. At nine o'clock in the evening of the same day, she had retired to her room, and was heard by the two occupants of the adjoining chamber to be gagging and choking so violently and in such a peculiar manner, that one of them knocked at her door and inquired if there was anything the matter with her. She returned an evasive answer, and remained apparently quiet throughout the night. She kept in bed the next morning, and refused her breakfast, but attracted no special attention until nine o'clock, when the same sound of gagging and choking was heard in her room a second time. In the course of the succeeding two hours an hysterical spasm came on. It was then ascertained that she had taken poison, and Dr. Hershey was immediately sent for.

He arrived at 11 o'clock, A. M., just fourteen hours after the first dose had been taken, and two hours after the second. At this stage of the proceedings nothing definite could be learned from her admissions or complaints. She lay in a state of partial cataleptic stupor, occasionally varied with slight muscular spasms such as are common in ordinary hysteria, and seemed to be unwilling or unable to answer questions. The customary effects of irritant poison were so entirely absent, that Dr. Hershey was induced to order an anti-spasmodic potion of some aromatic tincture in camphor water, on the supposition that the attack was a purely nervous one alone. Of this draught she took some four tablespoonfuls, at intervals, in the course of two hours. According to her own repeated assertion, which there is no reason to disbelieve, it was the first fluid which she took into her stomach during at least thirty-six hours—that is for twenty-four hours before and some twelve or thirteen hours after the taking of the evening portion of the arsenic. No change beyond the abatement of the hysterical symptoms occurred until one o'clock in the afternoon, about two hours after she had swallowed the first of the camphor water, when the doctor was hurriedly recalled on account of the sudden onset of violent pain and vomiting. The most frequent, and in this instance, at that time, the only positive, symptoms of arsenical poisoning had at last presented themselves, sixteen hours after the first powder had been swallowed, and four hours after the second.

The fresh hydrated oxide of iron was immediately given and continued, in divided doses, to the amount of five ounces; but notwithstanding this and the free use of sulphate of morphia and cold mucilaginous drinks internally, and of depletion with cups and the subsequent application of cataplasms and a blister externally, the pain and vomiting increased in severity and frequency until the afternoon of



Sunday, the third day. She then appeared to be so utterly prostrated that no hopes were entertained of her recovery either by herself or her physicians. A paper containing a portion of the white arsenic of the shops had already been found in her room. The apothecary had been visited, and had confirmed her story as to its purchase on the preceding Thursday. The amount remaining in the packet had been compared with the quantity stated by her to have been swallowed. Questions were again asked her, when she was supposed by all to be in extremis, and were answered as frankly and fully as could be desired. These answers being given under the impression that she was at the point of death, are fairly entitled to belief for that reason alone; but they were corroborated wherever possible by other testimony, and invariably supported by her own statements in answer to the repeated and varied cross-questionings to which she was subjected by her medical attendants during convalescence, and after every inducement for a perversion or suppression of the truth had been removed.

The history thus gathered from the patient and her friends, was, in a few words, that her whole mind had become absorbed in the thought of her troubles, and the desire to end them in the grave; that with this intent, having secretly provided herself with arsenic, and retired to her room after a whole day of fasting and agitation, she attempted to swallow a teaspoonful of the dry powder, and was so irritated in the throat by it as to alarm her neighbors. She coughs out a part of it, but manages to retain about half-a-teaspoonful. She lies down, as she expects to die, but spends the night without any change whatever, and, as she thinks, without sleep. The next morning she swallows another half-teaspoonful in the same manner and with the same difficulty. Soon after this last attempt, she loses her self-control, becomes hysterical, and announces her probable fate to the family who come to her aid. During all this time, however, she feels no nausea, thirst or pain, and has not the slightest idea of the agony she is shortly to endure. It is not until after she has taken several drinks, that she begins to realize the horrors of her situation. Her entire ignorance of the effects of arsenic was fully demonstrated, and of itself afforded satisfactory proof that, although intelligent and tolerably well educated, as well as a determined girl, she could not possibly have simulated her disease, or so contrive to regulate the dose as to effect a partial poisoning only. The symptoms, however, were too well marked in their course and violence to admit of any doubt as to the existence and progress of an irritant poison. Nothing more was required to demonstrate the presence of arsenic, except the chemical examination of the matter thrown up from the stomach, and of the feces and urine. Unfortunately, this examination was not made. My attention was first called to the case on the evening of the third day, Sunday. The vomiting and pain had then ceased. Reaction had begun, although the patient was still in a partial collapse, with feeble, quick pulse, pale sunken countenance, cool, moist skin, great muscular debility, and a tendency to hebetude with occasional temporary cataleptic spasms. The pharyngitis, tympanitis and epigastric and hypogastric tenderness were very well marked. These gradually subsided, and were accompanied and succeeded by similar evidences of inflammation running along the course of the colon down to the rectum, and going off with tormina, tenesmus, bloody stools, hemorrhoidal irritation and strangury, and followed for a few days by an acne-like eruption of the skin.

The treatment, which was demulcent, anodyne and cautiously supporting, with a gradually improving regimen, need not be particularly detailed. The youth and general health of our patient secured her a comparatively rapid convalescence. By the end of five or six weeks, she had recovered her former flesh and color, and appeared to have escaped with unimpaired gastric and intestinal functions. The only difficulty that remained, was a debility of the lower extremities, amounting to a partial paraplegian, and a relaxed condition of the pelvic organs, which led, on two different occasions, to a painful retroversion of the uterus. Both of these infirmities, however, were so far removed in the course of another month, that, at the latter part of June—within three months after her attempt—she considered herself perfectly restored to health.

It was of course impossible to determine the amount of arsenic actually swallowed in this instance. A sufficiently near estimate, however, was reached through

the patient's own statement, made under circumstances that afforded no inducement for deception, and confirmed to a considerable extent by her fellow-lodgers, and by the apothecary who furnished the packet from which the two portions must have been taken.

Supposing her to have retained no more than three quarters of a teaspoonful altogether, which is the smallest probable amount, she must, according to the approximative measurements given by Taylor, have been exposed to the destructive action of at least one hundred and twelve grains in all, and to that of fifty-six of these grains, thirteen or fourteen hours before the appearance of any nervous symptoms, and just sixteen hours before the accession of pain, vomiting and thirst.

The cerebro-spinal demonstrations presented nothing sufficiently different from those of ordinary hysteria to warrant their being regarded as the narcotism sometimes produced by arsenical intoxication. Still, allowing these disturbances to have been the premonitory signs of more serious poisonous action, we yet have an interval of fully thirteen hours' duration, after the ingestion of nearly a drachm of the acid without the advent of a single irregular sensation.

Cases are reported in which arsenic has begun its work at once upon its victims. More frequently, a few minutes have elapsed; but the average period at which the deleterious operation generally begins to manifest itself, is stated to be from half an hour to an hour. Taylor refers to a case communicated to him by Mr. Todd, in which no symptoms appeared for two hours, after one drachm had been taken on an empty stomach. Orfila relates one in which five hours passed, after more than half an ounce had been taken, without decided mischievous effect; Devergie reports an interval of four hours; Taylor quotes another of nine hours duration, and one from Belloc, (see Taylor On Poisons, p. 259,) in which the symptoms did not present themselves for ten hours. This last is the longest instance of uncomplicated freedom from noxious symptoms hitherto on record. There are others besides these, of different periods of protraction, which need not be detailed. The case of Tonnelier, (see Orfila, Toxicologie, 1,386,) although very remarkable, is not entirely free from doubt, as an instance of retarded progress in arsenical poisoning. That of Clegg, also, (see Taylor, Med. Jurisp. 76,) can hardly be included in the class, because it must have been affected by the modifying influence of opium upon the arsenical irritation. I have not been able to find, in the Journals, any analogous cases not already cited in the works on medical jurisprudence; and I am, therefore, inclined to believe that our patient stands alone in her experience, so far at least as the published annals of toxicology have gone.

The bearing of such experience on the medical evidence in certain criminal cases, may be easily understood. Christison expressly recognizes the importance of it in his work on Poisons, (p. 337,) and refers to a trial, in which a woman escaped conviction, principally because "the symptoms of poisoning did not begin until more than eight hours after, the only occasion on which the prisoner was proved to have administered anything in a suspicious manner." "As I was not at that time," continued he, "acquainted with any parallel case, except that recorded by Orfila, I hesitated to ascribe the symptoms to the draught; and, consequently, as the other medical witnesses felt the same hesitation on the same account, the proof of administration was considered to have failed. I am not sure that I should have now felt the same difficulty."

Our case seems to me to possess considerable interest, also, in a physiological and pathological point of view; but as my object is especially to report the observation for what it may be worth in questions of medical evidence, I must leave to the ingenuity of other inquirers, all speculations as to the cause and nature of the delay of gastric disorder under the peculiar circumstances stated.

*Medical Examiner.*

CASE OF YELLOW FEVER OCCURRING IN ST. LOUIS, AUGUST, 1855.—By Wm. Webb, M. D.—The subject of the contagion and importation of Yellow Fever, still being debatable with the profession, it behooves us to put upon record, all such

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## Epidemic Typhoid Pneumonia amongst Negroes.

*By D. WARREN BRICKELL, M. D., of New Orleans.*

"In writing the history of a disease, every philosophical hypothesis whatsoever, that has previously occupied the mind of the author, should be in abeyance. This being done, the clear and natural phenomena of the disease should be noted—these, and these only. They should be noted accurately, and in all their minuteness; in imitation of the exquisite industry of those painters who represent in their portraits the smallest moles and the faintest spots. No man can state the errors that have been occasioned by these physiological hypotheses."—SYDENHAM.

Implicit faith in the correctness of the above great principle, laid down by the illustrious Sydenham, leads me to adopt a somewhat clinical style in illustration of the Protean disease I am about to consider. It may appear somewhat tedious to the cursory reader, but to the critical inquirer after truth in her minuteness, there will appear nothing supererogatory.

I make no pretensions to originality in that which I am about to write: I know that more skilful hands have worked in the same mine: yet I hold it a duty to "contribute my mite to the treasury of physis" whensoever it may appear to my humble judgment expedient. I offer no vague theories, preconceived or deduced; I record facts as they have been evidenced to my senses, and upon them base my humble suggestions.

Prof. Saml. Henry Dickson of South Carolina says, in his "Practice," that "the first notice we have of its (Pneumonia Typhoides) appearance was in the year 1806, in Medfield, a town of the State of



Massachusetts, whence it spread gradually"—Again, "In the winter of 1815, it first prevailed in this State (South Carolina,) and was then, and more widely in 1816, epidemic".—Again, "It has not advanced further south than the State of Georgia (1845,) nor has it been familiarly known beyond the latitude of this City" (Charleston).

Reposing great confidence in the accuracy of the statements of Dr. Dickson, I make the above extracts for the purpose of reminding the reader that Typhoid Pneumonia is comparatively a new disease amongst us. True, some of my senior brethren may exclaim against my pronouncing that *new*, which they may have been accustomed to see frequently during thirty or forty years: yet, for all that, Typhoid Pneumonia is new. When we look back through the long vista of years, at the supposed birth day of Medicine as a unit, and now behold her scarce yet able to "stand alone," shall we dare say that the disease of fifty years is more than in its early infancy? How long this disease has existed South and West of Georgia, I have not been able to inform myself; but, supposing, that it appeared amongst us soon after Dr. Dickson wrote (1838,) we will find that it has had an existence here of less than twenty years. Moreover, according to Dickson, our transatlantic brethren are comparatively strangers to the disease, inasmuch as their first notice of it is about the year 1838.

I do not propose to enter into a history of the disease farther than the extent to which I have seen it; I merely mention the above facts for the purpose therein stated, as well as to show that my "hobby" is by no means an antiquated one. I proceed to the introduction of a few cases illustrative of the malady in question. My observations have been almost wholly confined to its prevalence amongst negroes; hence the title to my article.

### DESCRIPTION OF THE DISEASE.

CASE 1.—Here lies before us the man Jim—18 years of age—likely and quite athletic—has rarely been sick during his life—has been at work in the field all the morning, and has this moment entered the hospital.

"What is the matter with you, Jim?" "Nothing, Sir." "Well, why have you come here?" "The order of the overseer, Sir, is to come to the hospital the moment we feel the least unwell: I felt a little *tired* just now, and thought I ought to obey orders." "And you say there is nothing the matter with you?" "Nothing, Sir, only I feel a little weaker than usual."



Let us examine the patient thoroughly. His pulse is feeble and considerably accelerated; his skin is rather dry, but of natural temperature; his tongue looks well; he is not constipated; his urine is secreted as usual; it is with great reluctance that he goes to bed, being fully convinced that he is not *sick*. But there is a certain something in the countenance of Jim which defies description, yet which speaks loudly, fearfully! It utters but one monosyllabic word, and that word is *death*! Our prognosis precedes our diagnosis:—his hours are numbered. Notwithstanding this, however, we are unable to detect any existing or even directly threatened local disease, and he really begs you to believe that he is *well*.

Six hours have elapsed however, and we return to the patient. We find him with a pulse becoming every moment more feeble and more frequent; his skin is cool, dry and shrivelled; his countenance says he has been sick at least ten days. He answers our questions reluctantly; is entirely conscious, but indifferent to all around him; makes no complaint, and says he feels no pain or distress of any kind—only feels somewhat exhausted. We again subject him to the most careful general examination, and on passing the stethoscope over every square inch of his chest, we ultimately find that which gives name to his disease: we find on the posterior portion of the middle lobe of the right lung, a spot about the size of a half dollar, which sends forth the *rale* of Pneumonia. We feel relieved; we have made a diagnosis; the man has Typhoid Pneumonia.

Six hours more elapse, and we re-apply the stethoscope; when, alas! the half-dollar spot is lost in the startling progress of the disease; the *half* of the right lung is entirely suffused, and the *rale* is now detected in the *left* lung.

Eighteen hours from the commencement of the attack, (the entrance of the patient into the Hospital,) we visit him again; both lungs are now extensively involved; he is panting for breath; but neither now, nor at our previous visits does he ever cough or expectorate at all; his pulse is like a thread and extremely rapid; his skin is cool and clammy; his countenance is ghastly. "What is the matter with you, Jim?" "Nothing, Sir." "How do you feel?" "First-rate, Sir."

In twenty-four hours from the time of his first becoming tired, Jim is no more! He has died of Typhoid Pneumonia. Percuss his chest before dissolution takes place, and there is a moderate degree of resonance throughout; auscult them, and throughout their entirety you find air bubbling through the effused liquid: there is no *solidification* of the organs.

Not being an artist, of course a finished picture can not be expected of me; but I think I have given a faithful representation, in a rude way, of that which I conceive to be *next* to the worst form of Epidemic Typhoid Pneumonia. During the same epidemic, I have known a patient to die in a shorter time with precisely the same train of symptoms as those detailed above, *except* such as were referred immediately to the *lungs*. I did not examine this patient after death, but I am certain that I should have been able, during his illness, to detect an amount of inflammation, or congestion, of the lungs sufficient to produce *death*; yet I affirm that, to my ear, there existed a perfectly normal respiratory murmur throughout both lungs up to the time of dissolution. I conceive this patient to have died from the exceedingly depressing influence of the epidemic cause on the nervous system generally, and before there could be a direct localization of the disease. This same thing I have known to occur in an epidemic of cholera. Negroes would lie down and die in a few hours, without either the vomiting, purging or perspiration peculiar to cholera. Whether such a case as that just mentioned could be classed under the head of Typhoid Pneumonia, I will not undertake to discuss here, though I feel perfectly satisfied as to the death having been caused by the same epidemic influence at that time prevalent.

CASE 2.—We find in the Hospital the man Peter; he has just come in from work, and goes to bed reluctantly; would much prefer sitting up by the fire, as he does not consider himself *sick*. “What is the matter with you, Peter?” “Well, Sir, I think I have a little fever; I feel a little weak, and have some pain in my limbs.” His pulse is rather feeble and considerably accelerated; his skin is cool and dry, and his tongue is pretty heavily coated and rather pale; his respiration is uninterrupted, and even slower than natural. In the course of conversation with him, however, he discharges his sputa, which we find slightly brownish and almost entirely devoid of tenacity—in fact, *watery*. This symptom leads us at once to his lungs, and auscultation reveals a loud crepitant rale over a surface two or three inches square in one lung—always on the posterior portion. He does not cough; has no pain whatever in his chest, and laughs at the idea of your pronouncing the existence of disease there. The only reason why he ever expectorates the matter before mentioned, is because you have forced him to talk (to which he is averse,) and in the course of conversation, there is occasionally a slight convulsive movement, not amounting to *cough*, which slowly brings it to his mouth; and even then, he is more apt to swallow than to discharge it, such is the state of utter apathy which possesses him.

This patient is placed under treatment. Under favorable circumstances, the progress of the disease may be arrested here, and at the end of six to ten days he enters into convalescence; otherwise, there is a gradual extension of the disease; the opposite lung soon becomes involved; the pulse becomes more feeble and very much accelerated; the skin dry and shrivelled, the voice husky; the urine is partially or entirely suppressed; the patient never complains, but ever insists that he is doing finely; until from the fourth to the fifth day, complete suffusion of both lungs takes place, and the man dies from suffocation.

In this case percussion before death yields "dullness," though never "flatness," and auscultation readily detects the air bubbling through *liquid* everywhere. In some cases which were protracted to seven or eight days, there has been found "flatness" and total absence of respiratory sound at the posterior or inferior portion of the lungs (according to the position adopted by the patient,) but this appeared to me the result of said position—a mere mechanical stasis of the effused fluids.

CASE, 3.—The man Sam has just taken to his bed; his most prominent symptom is a dull, heavy pain in the posterior portion of the chest, and directly between the scapula; and this pain is somewhat increased by a full inspiration. His pulse is somewhat accelerated and feeble; his skin is cool and harsh; he does not complain, but when you question him he directs your attention to the pain, yet says that it would not, of itself, have stopped him from work, had he not felt rather feeble.

We examine the chest, and readily detect the crepitant rale to a considerable extent over the posterior portion of one, sometimes both lungs. He coughs quite frequently, especially when in conversation, and up comes dirty brownish sputa, moderately tenacious, but by no means capable of adhering to the bottom of an upturned vessel. The patient's respiration is somewhat hurried too, and he seems to be aware that he is sick: indeed, his countenance is to a certain extent indicative of anxiety.

Under favorable circumstances, this case will be convalescent in ten or twelve days; otherwise, a gradual extension of the disease takes place, and he dies from the seventh to the twelfth day.

I must not omit to mention here the very important and singular fact, that the condition of the two last patients was that in which they were found soon after coming out of the field; showing clearly that they had been laboring during the actual existence of the disease—not from compulsion, but from choice, as there was a peremptory standing



order to the effect that no sick person should labor for one moment after he or she should make known the fact.

CASE 4.—We now come to the consideration of a class of cases of which I am happy to say I have seen but one, and I hope never to see another. I refer to the anginose variety.

One of the most athletic negro men I ever saw was taken with slight catarrh, accompanied by slight febrile symptoms: living at a point two miles distant from the nearest infected quarter (although belonging to the same owner,) I did not for one moment suspect him of being a victim to the same epidemic influence: in twenty four hours, however, I perceived that his symptoms were of a decidedly Typhoid nature, and I as clearly preceived that his days were numbered. Suffice it to say, that in spite of all that I could do for him, he died on the fifth or sixth day with all the terrible symptoms peculiar to the most malignant form of anginose disease, the lungs remaining intact throughout.

During his illness another servant in the yard was taken with the disease, though not in this form, and with difficulty recovered.

Dickson, in his Practice, says he has "seen a stout young man die in about six hours from the invasion of this form (the malignant) of Pneumonia Typhoides, and a young woman sink in about thirty six, in spite of the most energetic and best directed efforts for their relief." Now, I am satisfied that I have seen a man die in *nine* hours from the invasion of the disease—that is, from the moment of his first complaining; and I have seen a young woman (between three and four weeks after parturition) die in less than thirty six hours. I can safely say, that in the case of the man, the lungs were not in the slightest degree affected: in the case of the young woman, an autopsy was performed a few hours after death, by two other physicians, and both lungs were found solidified throughout nearly their entire extent.

In addition to the types mentioned, I have seen during the prevalence of epidemics of Typhoid Pneumonia, a few cases of well-marked, frank, sthenic Pneumonia—cases presenting all the usual symptoms, such as dyspnœa, suppressed cough, pain in the side, "brick-dust sputa," etc.; and for reasons which will be given hereafter, I have set such cases down as one type of the disease under consideration.

*Diagnosis.*—I have not unfrequently heard practitioners avow that nothing was more readily arrived at than the diagnosis of Typhoid Pneumonia, and that this prompt diagnosis is the hinge on which hang all the chances of success in treatment. "An early diagnosis and prompt



depletory measures to relieve the oppressed lung, and the disease is stripped of its terrors." Of such individuals I would ask, "On what ground could they possibly predicate a diagnosis of Typhoid Pneumonia in the commencement of any first attack on a plantation, in such cases as that of the man Jim, recorded here as "Case No. 1?" After the epidemic fully declares itself, it does not require a Solomon to say that any and all cases may be classed under the one common head.

The diagnosis in case No. 2, is more readily arrived at; and yet with the inexperienced observer it is by no means a matter of certainty. I have characterized the discharge of sputa in this case as "accidental," and this is the hinge on which the whole thing hangs in the beginning. Nothing but the fact of our forcing the patient into conversation, will, as a general rule, give rise to expectoration; and how many young practitioners are there who would, from the apparently mild character of the general symptoms, be too sparing of their interrogatories, or, even should the patient carelessly discharge his sputa, would be too unsuspecting to examine them. I have had a very intelligent unprofessional man to laugh outright when I pronounced death to be the inevitable doom of such a case as Peter; yet the patient died almost while he laughed.

In the case of Sam, (No. 3,) our diagnosis becomes comparatively an easy task, if we have at all familiarized ourselves with the art of physical exploration of the chest. The patient at once directs our attention to the part affected; he tells us there is pain in his chest, and of course we apply the ear to the spot designated. And yet we must not always expect to find the rale at the spot where Sam says the pain is. I have more than once been nonplussed in just such cases. Instead of the expected rale, I have found a perfectly healthy respiratory murmur, and yet the patient would swear to a pain in this particular spot, and even complain of a *stitch* when directed to take a full inspiration. If we will place the ear on a corresponding spot in the *opposite* lung, we will find the rale. This is a thing which I have never witnessed in the white man; but it is so common among negroes, that I mention it for the especial benefit of those least tinctured with suspicion, or who are not yet fully acquainted with the negro character. For my own part, it has been my misfortune to see so much of this terrible disease, that I am never satisfied until I have thoroughly examined both lungs in all cases; in fact, it has become a habit with me to apply my ear to a negro's chest, no matter what he complains of, if I am called to see him at any time between the months of October and May.

It seems rather strange that we should apply the term, Typhoid

Pneumonia, to such cases as Nos. 5 and 6; the one being strictly anginose, and dying without pulmonary symptoms; the other dying at the end of nine hours, and with the whole respiratory apparatus intact. For my own part, I am opposed to this loose system of nomenclature; still, as the fact of their common *cause* has induced the adoption of a common name, which seems to be acceptable to the profession, I attempt no innovation. Certainly neither of these cases could be diagnosed Typhoid Pneumonia *per se*, and from its own inherent characteristics. However, as our treatment is almost altogether indicated and regulated by the general symptoms, it matters little whether we name the disease or not.

*Prognosis.*—All diseases are said to be more dangerous in an epidemic form, and probably no rule is better established. Yet there are prominent exceptions. Who, for instance, has not seen a mild epidemic of Scarlatina, or of Rubella, or of Pertussis? And yet the rule to the contrary is so strong that we all feel a dread of encountering these diseases in an epidemic form. I have never witnessed a mild epidemic of Typhoid Pneumonia, yet I can conceive of such a thing—indeed I am satisfied that there are physicians who have never encountered any other *than* mild epidemics of the disease. I have attended sporadic cases, and have invariably found the results of treatment much more satisfactory;—in fact, I have never lost a sporadic case. Still, I am not prepared to say that these cases are any *milder* in their nature; I rather think I can attribute the favorable results to other and very reasonable causes—absence of the effect of panic, and the far superior system of nursing.

In the consideration of this disease as applied to our slave population, however, he who has opportunities for personal observation cannot be at a loss for causes which are not only always present, but, while they exist, must ever tend to complicate and render more grave the prognosis. I must be candid enough to say that it is with no ordinary degree of anxiety that I look forward to the day when epidemics amongst our slave population will be largely stripped of their terror by the introduction of a system of medical police both simple and economical in its nature, yet which is nowhere to be found amongst us in a state even approaching the degree of perfection to which it might easily be brought. I contend that the general prognosis, not only in Typhoid Pneumonia, but in all the epidemics to which our negro population is subject, is seriously effected by the rude and often careless manner in which everything pertaining to their management is conducted. We have but to look at the great cause of mortality in our public hospitals

for whites (neglect previous to entrance,) and we have found one of the causes of the great mortality amongst negroes: combine with this the loose system of nursing, and the inattention to general hygiene during epidemics on plantations—and the mind that is capable of grasping more than one idea at a time will no longer be at a loss to account for unsatisfactory results. But more of this under the head of “treatment.”

I think I may safely assert that the prognosis in such cases as that of Jim (No. 1.) may be set down as almost universally unfavorable: I firmly believe that nearly every such case will die, no matter what you do for them, or how soon you reach them. All Physicians know that there are certain cases on the features of which the Great Hand has impressed the seal of death from the very earliest moment of their indisposition, and notwithstanding we most justly conceive it to be our duty to use our best efforts to avert the impending stroke, still we do so with the conviction that death must be the inevitable result. For thirty days together have I toiled amidst this disease; I have exhausted the *Materia Medica* in combating it, but I have never known a case to recover, to be for one moment checked in its rapid progress to death, when presenting the symptoms detailed in the case No. 1.

Such cases as the one mentioned as having died in nine hours, I regard as the worst that can occur, and totally beyond the reach of medicine. Having seen but one of the auginose variety, of course I am not prepared to form anything like an accurate idea of the chances for life in this class of cases, though I am inclined to place the prognosis on the same footing as that of No. 1.

In such cases as No. 2, our prognosis is extremely doubtful; indeed I think I may safely say, that from thirty to fifty per cent. of them will die; this percentage, of course, regulated altogether by the system of nursing adopted, and the stage of the disease when treatment is first commenced. In the symptoms detailed, we cannot fail to recognize a disease both malignant and insidious; its malignancy, of itself, would give the practitioner great concern, but when we add to this the latter characteristic, we have a most formidable enemy to contend with. In ninety-nine cases out of one hundred, the negro himself places no stress on the great and important symptom of accession—lassitude—and, under the loose system of medical surveillance usually adopted, the overseer or owner, deceived by the comparative tranquility of the patient, treats him too long, or does not treat him at all, and when at last the Physician is called in, it is but to receive on his broad shoulders the



responsibility of his death. I can now call to mind seven or eight cases of this kind, which have occurred in my practice, where the owner or overseer has been *astonished* when I would pronounce death the inevitable doom of the patients whom they had treated twenty-four or thirty-six hours previous to calling me in, and whom they had never for one moment regarded as seriously sick. One would suppose that dire results like these would deter unprofessional men from assuming such awful responsibilities; but while vanity and parsimony exist as human characteristics, just so long, I fear, will medical men be subjected to this most disagreeable and unprofitable sort of practice.

In cases like No. 3, the prognosis is far more favorable. Here the disease itself is not only more amenable to treatment, but the very complaints of the patient more readily direct the attention of the owner, overseer or physician to the real nature of the affection. If the Physician is called in early, all other things being favorable, he can treat the case with something approaching certainty; and even if the patient has been neglected or quacked for twenty four hours, still the chances for affording relief are better than in either of the foregoing varieties. And yet we must not forget that even this is a very formidable disease; and, taking into consideration the usual known condition of such patients, as regards food, clothing, and, above all, *nursing*, I believe that as many as fifteen per cent will die.

The prognosis, then, in Typhoid Pneumonia, as I have seen it, presents four different phases. Cases such as the one mentioned as having died in nine hours, will invariably die; those like No. 1, may possibly recover, though I have never seen an instance of even an effort at recovery; those like No. 2, stand pretty nearly equal chances of death and recovery; and those like No. 3, have about eighty five chances in one hundred of recovering. These are the results to be anticipated under the existing state of medical police; that a great change for the better could be effected admits of no doubt in my mind.

*Pathology.*—I regret very much that my opportunities for studying the Pathology of this disease through the medium of the scalpel have been so limited. Owing to the utter abhorrence which negroes have of “cutting up” a dead body, and the consequent, and very manifest bad effect exerted on their minds, I have but twice been permitted to make a post mortem examination. In one instance the body of a young woman, who died within thirty six hours, and with all the symptoms of that which would more properly be considered *Sthenic* Pneumonia, was opened by two medical friends, and both lungs were found nearly



entirely *solid*. In the other instance the patient died with all the symptoms of *Asthenic* or *Typhoid Pneumonia*, and here I found the lungs throughout almost their entirety, suffused with a slightly tinged and almost aqueous fluid: with no apparent thickening of the lining membrane of the air tubes, and the parenchyma of the lung presenting a healthy appearance when freed of this fluid by moderate pressure. A piece of the lung thrown into water would float *heavily*; but if first squeezed moderately, and then thrown into water, it would float as lightly as the healthiest lung.

Something of an apology might be required of me for forming an opinion on data so limited as the foregoing; nevertheless it is so, though with some modification. On making the second autopsy mentioned, I most fully verified my preconceived idea of the real Pathology of true *Asthenic* or *Typhoid Pneumonia*. Auscultation and percussio had taught me that *hepatization* of the lung never occurred, and that such apparent solidification as is sometimes to be found on the posterior portion of the lungs is simply a stasis of fluids consequent on position. There is no proper inflammatory condition of tissue such as is found in *Sthenic Pneumonia*, but rather a sort of passive congestion. In malignant *Typhoid Pneumonia* the only effort at *inflammatory* action is found as a secondary affair altogether. When the patient has been very low, where the lungs have been very much obstructed, I have seen the convalescence seriously interrupted by one or more attacks of limited *Pneumonia* proper. In such cases I have seen the patient expectorate the "brickdust sputa," and complain of dull pain in the region affected. I have always attributed such results to the mechanical irritation produced by great quantity of fluid effused in the original attack.

After all, however, I regard the pulmonary symptoms in this disease as secondary in every sense of the word. I have so often seen patients come in sick, during epidemics of *Typhoid Pneumonia*, and lie with all the general symptoms well developed for twelve or even twenty hours before any local lesion was discernable; and, more than this, have seen them actually *die* with only these symptoms ever developed, that I am forced to think that our attention should be directed to something beyond the lungs as the point of attack. How to define this something I know not, but there is an effect produced on the whole nervous system of the patient which is so palpable and so serious in its characteristics, that we are led irresistibly from the lungs to it.—What the cause of this effect is, remains a mystery. That it is atmospheric I have no doubt, but this is all we can say of it. As proof of its atmospheric nature,

I will mention here, that I have, on several occasions, known negroes, coming from healthy localities three to five miles distant, to take the disease in all its malignancy within twenty four hours after visiting the infected quarter. Some of these negroes remained in the quarters; others only visited them for one, two or three hours. Negroes seem, too, to be peculiarly liable to contract the disease in this way. I have seen but one white person attacked by the disease whilst it raged among the negroes.

*Treatment.*—Under this head I shall only advert to my own individual experience. From the foregoing remarks it could readily be foreseen that I would direct my attention much more assiduously to general than local treatment, more especially in the primary disease. I freely confess that when I first came in contact with Typhoid Pneumonia, its name seriously misled me; I had been taught to entertain such a holy horror of all *inflammatory* conditions of the lung (this disease itself being considered inflammatory,) and had been so thoroughly impressed with the conceived importance of the application of cups, blisters, &c., that I for a time overlooked, to a great extent, the general condition of my patients, while I too confidently relied on affording relief through the means generally acknowledged as efficient in the treatment of true Sthenic Pneumonia—viz., depletion, both general and local. It was not long, however, before I discovered the error, and the following is the result of such experience as I have acquired.

I am confident that, in such cases as Nos. 1, 2, 3 and 4, all depletory measures are not only useless, but positively injurious. I cannot now call to mind a single instance of the kind, wherein depletion was resorted to, where decided injury was not the result. Some of these patients *can* be bled, but the blood which flows from them will oftener than otherwise even fail to coagulate; and in the graver class of cases I have frequently seen the blood *refuse* to flow from the veins freely opened in various parts of the body. I have seen the application of a few cups throw a patient into fatal collapse, and I have seen moderately free emesis or purgation do the same thing. In fact, in such cases as the above, every remedy, be its name or class what it may, which tends to depress the vital powers, even temporarily, is positively injurious. In the history of the foregoing cases I think it must be apparent to every mind that there is but one great absorbing indication, and that is to support, if possible, the extraordinarily depressed vital energies of the patient, gradually withdrawing your assistance as nature may seem

to be returning to her original independent condition. What then are the remedies best adapted to this indication?

I have not only employed the whole artillery of the *Materia Medica* in combatting this disease, but to the class of stimulants I have given especial attention. My object was to find a *permanent* stimulant, and, at the same time, one that would create as little real perturbation as possible. This could only be attained by experiment. All practitioners know that in the graver diseases, medicines frequently fail to produce effects analogous to those produced in the healthy subject, or even in one moderately indisposed, and in Typhoid Pneumonia this is eminently the case. I do not lay claim to any originality in this matter, but I think I can safely assert for quinine, in such cases as I have mentioned, a supremacy over all other remedies whatever. It is no *specific* however, inasmuch as there are too frequent instances of its failure. If I were to be confined to a single remedy, I would choose quinine, and yet it would be an unfortunate condition in which to be placed, as there are a few patients to be met with, who cannot take the remedy at all without consequent injury, and in the large majority of cases a few *adjuvants* to quinine may be introduced with infinite advantage. The principal of these are Brandy, Mustard, Opium and nourishing food.

As a general rule, those patients who bear the administration of quinine well, will derive advantage from the simultaneous exhibition of brandy, and the two together appear to constitute a better *permanent* stimulant than either alone. Mustard is of great assistance as an external application, and is frequently indispensable in arousing the patient from the lethargic condition I have before described. Opium is a valuable remedy in particular cases, but I have seen more harm result from its administration than from any other internal remedy. I would never administer it in other than cases of undue restlessness or insomnia. In such cases, in moderate doses, it frequently produces the happiest results; but I have seen nothing in its exclusive and free administration to warrant the praises, as an invaluable *curative* agent, which some practitioners bestow on it. My experience is, that in all cases where it is not *indicated*, as above, it is positively injurious. Its friends claim for it the appellation of *stimulant*, and give it in large doses accordingly. I have found it stimulant, it is true, but its stimulant effect is not only exceedingly transient, but is promptly succeeded by the most injurious of all depressing influences—I have sometimes been astonished to see what quantities of this drug some of these patients will take, and with



the utmost advantage; but in all such, one or both the previously mentioned indications were prominent.

Nourishing food is indispensable in this disease. While there are a few individuals who cannot take it, or who loathe it, and in whom it is consequently injurious, the large majority will take beef tea, wine wholly, &c., freely and with marked advantage. There is, as a rule, no contraindication; the digestive function is frequently very partially impaired at the very time when we find one or both lungs extensively involved, and the general symptoms very grave.

It will be seen that I have named but few remedies in the treatment of this disease, but this is not done to the exclusion of all others. I mention these few as the most reliable according to my experience; yet there are substitutes which must by no means be overlooked. Some patients cannot take brandy, but will take good wine with advantage. Others cannot take opium, but will be benefited by the exhibition of *Hyosciamus*, &c. The alcoholic extract of *Aconite* is frequently very soothing, and is the more valuable since it produces no disagreeable "after effects." For quinine I know of no substitute as a permanent stimulant. From the diffusible stimulants I have derived no advantage. At one time I gave a great deal of Carbonate of Ammonia &c., but I found that this class of remedies was not only more uncertain than quinine, but that it was impossible to procure nurses capable of administering them with judgment during the absence of the Physician.

In regulating the dose of quinine we have to be guided by circumstances. As a rule, the patients will take three or four grains every hour or two with the best results, but there are other cases in which ten grains every two hours is not too much, and this continued for as much as a week at a time. I saw a patient once take six grains of quinine every two hours for eleven days, and with marked advantage. Frequently during the time it was attempted to reduce the dose, or to remove the remedy entirely, and immediately bad effects were evidenced. In a good many instances the patients will complain of the tinnitus aurium produced by quinine, or will even be somewhat delirious; but these symptoms may readily be overlooked when its other effects are seen to be beneficial—viz., when we perceive the pulse increasing in volume and its velocity diminishing, the skin growing moist, the general capillary circulation being restored, and the respiration more tranquil.

The quantity of brandy to be administered cannot be defined. Each case determines its own requisite quantity, and the practitioner must be able to meet the indications. For my own part, I have to reproach



myself with not administering this remedy in sufficient quantity. He who administers table-spoonful doses of brandy to negro patients with Typhoid Pneumonia will miss the mark in three cases out of four. A better rule is to begin with a little too much, and have to reduce the dose, than to lose precious time by beginning with very small doses and having to increase gradually. I have seen a stout man and woman, who were apparently *in extremis*, absolutely require a third of an ordinary tumbler of best brandy every hour and a half or two hours. Less than this was insufficient, and under these large doses they recovered—and no beholder could for one moment doubt the efficacy of the remedy.

But one of the most important features in the management of Typhoid Pneumonia is *good nursing*. This is the turning point. No matter how favorable a case may be in the beginning, if it is neglected during the absence of the physician, neither quinine, brandy, nor opium, nor all combined, can effect any good. A case of Typhoid Pneumonia must be nursed like an infant; everything must be done *for* the patient. If he has inadequate bed clothing, or is allowed to eat what he pleases, or to get out of his bed, &c., &c., I believe medicines are little better than useless. The physician derives no pleasure or advantage from treating cases which are badly nursed, and the planter has not fulfilled his duty, either in point of humanity or self-interest, who allows his sick negroes to be badly nursed, or even not nursed at all. How often is it the case that the planter looks on quietly and *expects* of the physician that he is to be not *only* physician, but nurse and apothecary, and, perhaps, have ten or twenty miles of road between his residence and his patients. This undue taxation *may* do when there are but one or two patients, but the moment an epidemic declares itself on a place, and ten, twenty, or more patients are on the list, it becomes utterly impossible that one man can do either himself or his patients justice, and fill even the two posts of physician and apothecary. He who attempts to prescribe advantageously in such an epidemic, must have a mind free from other thoughts, and he must be allowed that amount of physical rest which will insure him against absolute fatigue. I do not know how it is with other physicians, but I have found it utterly impossible to do justice to the sick, during epidemics, when they are scattered about the quarters. When they are all brought to a hospital as soon as taken sick, they can not only be better attended by the physician, but by the nurses also. And besides this advantage to the sick, the well negroes who remain in the quarters are better off; they are not interrupted at night when they should be sleeping, and thus too frequently made sick; nor are they (if

the hospital is properly organized,) as is too often the case, made sick by being robbed of nearly or quite all of the bed clothing for the purpose of wrapping up the sick man of the family. What physician, who has had any experience in plantation practice, has not seen this thing done repeatedly? Who has not lost patients for the want of good brandy, comfortable beds and bedding and good nursing?

But, above all, we must condemn the almost universal practice, on the part of owners and overseers, of tampering with their sick negroes for one, two or more days before applying for medical aid. This practice *may* be in some measure excusable in the more common acute diseases of negroes, but during the existence of a malignant epidemic it is wholly unpardonable; for, laying aside all consideration for the reputation or comfort of the physician (which we all must feel to be too commonly the case,) they are thus depriving the patient of almost the only chance he has of recovery. A physician called to a patient thus quacked is in pretty much the same situation as a man called on to stay an already rapidly-falling wall—a timely prop might have readily stayed the tottering structure, but he who applies his shoulder to it while falling only runs the risk of being buried beneath its ruins. It is this unfortunate feature which divests “plantation practice” of its chief interest to the medical man, and causes so many of those engaged in it to relapse into a limited routine system of medication, which, under such disadvantageous circumstances, is the best they can do. The planter has no confidence in himself or his overseer in the matter of putting a new spoke in a cart wheel, and sends for a mechanic at once; but of the intricate mechanism of man, as well as of the principles which govern its motions, their knowledge is complete, or, at any rate, sufficient, in their own estimation, to warrant such efforts for the restoration of the sick man as they would not, on any account, have exerted on the simple and comparatively worthless cart wheel. That there are noble exceptions to this rule (for the custom is so general that it would seem to merit the title) admits of no doubt; all country practitioners meet with them; but they are comparatively few. I offer no apology for introducing the subject here—it is an all-important point in the matter under consideration.

I have hitherto discarded all depletory measures in the management of Typhoid Pneumonia; yet there are occasional cases occurring in these epidemics which do warrant this mode of treatment to some extent. I place these cases in the same category with Asthenic Pneumonia because of their occurring as above, but the palpable difference in the symptoms

will always afford the practitioner ample reason for difference in treatment. When my patients have been complaining of considerable pain in the side, when they were laboring under the constant suppressed cough, and were expectorating the tenacious "brickdust sputa," and presented a full and bounding pulse, then, and then only, have I seen general and local depletion do good. But there are many patients with what would be called "Frank, Sthenic Pneumonia," who present, at the same time, one symptom which my experience tells me ever contra-indicates depletory measures. I allude to a frequent and soft pulse. I have heard practitioners say that this pulse would "rise" and become less frequent under venesection: I can only say that I have never witnessed such a result.

The truth is, as a rule, negroes laboring under Frank, Sthenic Pneumonia, will frequently bear local depletion with advantage, but there are so few who, in accordance with my experience, bear venesection, that if I were called on to throw away any one remedy, I should say, let it be the lancet.

From the use of blisters to the chest I have seen great benefit result, but only at one stage of the disease. When the patient is reacting, as it were, from the state of great nervous depression already described, then the blister over the part of the lung affected seems to do good by promoting absorption of the effused fluids, as well as by counter irritation. From their application early in the disease I cannot say that I have seen any good result, while they are frequently very tardy, or even fail entirely to irritate the skin, thereby causing the physician to lose the benefit frequently to be derived from mustard; and they sometimes produce an amount of pain actually depressing to the patient. I have thought, too, that the application of a large blister to the chest, in the stage above mentioned, prevented those little attacks of acute pneumonia which I before alluded to as a frequent cause of tardy convalescence, or even sometimes of death.

There is one more remedy to which I will devote a few words. When the patient exhibits signs of reaction, in order to *anticipate* any torpor of the liver consequent on the state of general nervous depression, I give a moderate dose of calomel—three or four grains—and repeat, if necessary. I have never seen this remedy exert any direct curative action in the disease, as asserted by some, and it is only with the above view that I would administer it at all. Purgation by calomel, as sometimes advised, is ever injurious.

*General Remarks.*—I do not pretend to discuss the subject of the cause of epidemics of this terrible disease; all efforts on my part to this

effect have been fruitless. Where I have seen the disease prevail there was apparently most attention paid to the general welfare and condition of the negroes; and in every instance the disease has prevailed during the existence of that which is generally considered our best winter weather, viz., when it was cold and dry. I have seen it prevailing for a month at a time, and simultaneously, in three different quarters belonging to the same planter, while the negroes of his neighbors were perfectly healthy, and that notwithstanding these quarters were nearer to those infected than the latter were to each other. The disease broke out, too, on all three quarters simultaneously. I have seen nothing to induce me to regard this disease as *contagious*, but that it is infectious I entertain no doubt whatever.

It is a strange and unfortunate fact that the disease almost always attacks the very best negroes on plantations; young men and women are its principal victims—the former in fully one-third greater proportion. Children seem to be almost entirely exempt, and those who have the disease appear to resist it much more readily than adults. Old negroes comparatively rarely have it, but they succumb to it very readily when it does attack them. Until December, 1854, I never saw an old negro recover from the disease. In an epidemic on the Yazoo River at that time, however, I saw two old men recover from severe attacks.

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### Interesting Case.--Death from Suppression of Urine.

#### EFFECT OF UREA ON THE BRAIN.

[The following case is extracted from a private letter from a young physician to a professional friend in this city: as it was not intended for publication the names of the parties are withheld. We approve of the views taken by the author, and acknowledge our indebtedness for the contribution.—Eds.]

I was called to see a gentleman about sixty years of age, about night-fall. I found him in a comatose condition, and was informed he had been so for eight or ten hours. I learned that the evening previous he had been tolerably well, with the exception of a slight headache. This had not prevented him from conversing as usual. He retired to bed early, and in the morning complained of his head and of general uneasiness. He did not get up, but in the course of an hour or two his son-in-law assisted him into an adjoining room where there was a fire. He



laid down, but was not much noticed for half an hour, when on attempting to rouse him he was found to be insensible. Some tea was poured into his mouth, which he swallowed. He soon after began to sweat profusely, which his family interpreted to be a favorable symptom. Things went on thus until at night, when, as I stated, I arrived, and found the patient in a perfect state of coma. I endeavored by every means to rouse him, but without success—his pulse was rapid and feeble. On applying my ear to his chest I could detect a loud subcrepitant *râle* in the upper portion of the lungs, as though the bronchial tubes were partially filled with mucus. Some little sign of sensibility was manifested by the patient upon pressure over the region of the kidneys. I endeavored to make him swallow an emetic, but the power of deglutition had ceased. While examining his chest I was struck by the very strong smell of urine about his person and the bed clothes, and thought he had passed his urine in the bed; on examination I found I was mistaken. I inquired when he had last made water, and was told not since the day previous. I examined the bladder, and finding but little urine, concluded he had suppression, probably dependent upon inflammation of the kidneys. I had several enemata administered, but in consequence of relaxation of the sphincter they came away almost immediately, bringing but a small quantity of fecal matter. The hot bath, blisters to the neck, mustard to the spine, &c., failed to do any good; the coma increased, and about four hours after my arrival the patient was a corpse. Unfortunately, as is too often the case in the country, I was refused the privilege of making a *post mortem* examination.

There was, probably, in this case subacute inflammation of the kidneys, giving origin to the suppression. The blood became loaded with urea; there was an attempt on the part of the skin to relieve the system, which failing, the result was coma and death.

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## Clinical Lectures on Surgery.

By M. NELATON,

From notes taken by WALTER F. ATLEE, M. D.,

J. B. LIPPINCOTT & Co., 1855, pp. 755.

Happily for M. Nelaton his reputation is well established and he is a Frenchman. Had he been a *rising man* and had these lectures of which

we have the reports by Dr. W. F. Atlee, been delivered in English, nothing could have saved him; for all the mistakes would have been charged to the lecturer, and not, as in this case, to the reporter.

Now it seems to us, that a good clinical reporter should possess one of two qualities; either, he should be able to put down every word that falls from the lips of the lecturer, or he should be endowed with that rare faculty of seizing hold of the strong points of a case, and presenting to his readers a bold outline of what has been seen and heard. Dr. Atlee cannot well be ranked in either class. It does look like injustice to M. Nelaton to publish to the world these hasty notes of his lectures taken by one who is evidently not familiar with the French language—and we might add, nor with English either. The whole volume bears upon its face the evidence of carelessness in preparation for publication of what might really be a valuable addition to our surgical knowledge, if what is good were not so lost in labyrinths of involved sentences, as to render it impossible to extract the meaning of the reporter.

In the first chapter, which is devoted to the consideration of "burns, contusions," &c., he takes a rapid glance at the subject of inflammation, quoting some very interesting results from the recent experiments of Bernard, or the important part which the great sympathetic plays in the production of heat in inflammation. But he has not gone far enough into the subject to explain properly the opinions which M. Nelaton holds on one subject (that of Anthrax) in opposition to almost every other surgeon of eminence. PP. 35 and 36, speaking of the treatment of Anthrax, M. Nelaton says: "Let an Anthrax alone, there will be openings formed, pus and the core (bourbillon) will come out, and after that, it will heal; but if you make incisions into it, the edges of the incisions will separate very widely and the healing process will be very long" \* \* \* \* "and M. Nelaton said he declared most positively that practice showed that the only effect of incisions was to delay the cure." "M. Nelaton formerly taught that an anthrax would not cause strangulation of the tissues; and this is true, for the tumor extends under the skin, and is rather sub-cutaneous than cutaneous; the cavity is not not at all so disposed as to undergo strangulation. What is called the core is not an eschar, but a simple product of secretion, a false membrane which has been formed. On this account he had said that incisions were not useful, for Anthrax being an inflammation characterized by the secretion of the albuminous liquid, which becomes

hardened, incisions most assuredly could not put a stop to this inflammation and the pseudo-membranous effusion which follows it."

Now Chelius defines a carbuncle to be "an inflammation of the skin and underlying cellular tissue, with considerable hard swelling, which runs into gangrene throughout its whole extent," and "the local treatment," he says, "consists in making very early a cross cut of sufficient depth, by which the constriction of the cellular tissue is removed." In fact, we have always thought that few points in surgery were better determined than the employment of early incisions in Anthrax, a treatment recommended by almost all surgeons, American or foreign. An opinion, therefore, so totally at variance with the recorded experience of so many eminent surgeons should at least be supported by some proofs, for the mere assertion of no one man, however elevated his position, is now taken without examination. That day has fortunately passed in medicine.

The Chapter 2d, on "Affections of the Blood-vessels" is very good, especially the article on "Varix," which is interesting on account of the condensed history of the different modes of treatment which have been tried at various times, and valuable for the formula for the preparation of perchloride of iron, used in injections in aneurisms and varix.

We would call Dr. Atlee's attention to the fact, that in giving the size of objects by comparing them with those that are familiar, it is better to select something the dimensions of which are fixed. On page 148, speaking of abscess in the popliteal region, a tumor is described as being about as large as a "penny loaf," which in France may possibly convey to one a definite idea; but here the size of our bread varies with the price of flour, and the comparison is not much more accurate than if he had said, "a piece of chalk."

Two excellent tables of the signs of luxations are given in Chapter VII.

Dr. Atlee says, on page 305, speaking of the death of a patient operated on for cancerous affection of the femur, "M. Nelaton was very much affected by the fatal result in this case; things had come to such a point," he said, "that really he was forced to ask himself the question, whether, morally, he ought to undertake such operations in his hospital; and it was not the only one in Paris where the mortality was so frightful; radical reforms were needed in all. Of all cases operated upon by amputation, in the preceding twelve months, only two had been saved." We sincerely hope, for the honor of French surgery, that there has been some misunderstanding of this statement, for we cannot conceive that

the gentlemen who are now at the head of the Parisian hospitals would remain inactive a moment with such an awful mortality staring them in the face. They would, before this, have discovered the cause and applied the remedy. If true, M. Nelaton may well say that reforms are needed, for we doubt very much if a like mortality has ever followed the amputations of any army surgeon, even when performed under all the disadvantages incident to military hospitals in time of war.

We meant to examine carefully each chapter, for we expected a great deal from the title of the work, but find so much to condemn that we give up the task. The work may yet be made valuable, and a credit to Dr. Atlee; but it must be first carefully written in English, leaving out a multitude of French idioms, which cannot even be translated. The great fault of the book lies not in the matter so much as in the style, which is obscure, and the whole is ill-arranged and badly got up by the reporter. It forms one of a class of works which we consider a disgrace to our profession, we mean *edited* books. As soon as some distinguished physician abroad writes a book, in which he has recorded the results of twenty or thirty years' study and observation, it is seized upon, hurried through an American editor's hands, who puts his name upon the back of the book, and modestly tells you, in the "Preface to the American Edition," that his additions are in brackets, with his initials attached.

In the olden time men first observed, then practiced, and then wrote: *mais nous avons change tout cela*—for now-a-days we first write, then teach, and finally practice, if we can.

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## Review---Physiological Chemistry:

By Professor C. G. LEHMAN,

Translated from the Second Edition, by Dr. G. E. DAY, of the University of St. Andrews, 2 vols Philadelphia, Blanchard & Lea, 1855.

(Continued.)

In continuing our observations on Dr. Lehman's very interesting work, we are much struck with the fact first observed by him of the presence of Hippuric acid in the urine of Diabetic patients. This is remarkably interesting, as Hippuric acid being a means by which carbon is evolved from the system by the kidneys when the function of the lung or liver is interfered with, as in the disease at present under



consideration, both these organs are unfitted for their normal duties. On the other hand, we see in the excretion of this substance, (which next to the bile is the richest in carbon of all the excretions of the body) an additional reason for the rapid emaciation of the patient. Our space will not permit us to enter into the very interesting speculation which this fact might give rise to and we are constrained to pass rapidly over the chapters devoted to the other constituents of the urine, which may be read with the utmost advantage by the practitioner and student.

With regard to the crystalizable substances of the blood, Dr. Lehman differs somewhat from the opinions of other chemist. He does not like, Robin, believe the presence of serum to be essential to their formation, and he considers that these crystals which have been derived from the serum alone are not true blood crystals, but are simply mineral salts. His observations on this subject, which is yet *sub judice*, are so pertinent, that we cannot do better than transcribe them. He says "I am inclined to believe that this crystalline substance, is not a mixture of a pigment and a protein body, but a pure chemical compound; the difference in the form of the crystals of different kinds of blood seems to indicate with tolerable certainty, that this compound must, however, be either a salt like, or a conjugated compound. All the analyses which I have hitherto made of the pure substance have failed, like all previous elementary analyses of the protein bodies, in yielding any definite views as to the constitution of this substance, but it seems to me that its recognition is rendered very simple on the supposition of a conjugation. If the somewhat irrelevant question were asked, whether the crystalline substance is contained as such in the blood corpuscles, existing in it only in a dissolved form, I could not directly affirm that such is the case, for the influence of such forces as light and oxygen which are necessary for the formation of crystals, is inconceivable without the co-operation of chemical action, hence we might be led to assume that an oxydation had previously taken place. As however, crystals cannot be found without the co-operation of carbonic acid, mere oxidation cannot constitute the sole form of metamorphosis of the substance, for carbonic acid must essentially contribute towards the production of the new substance, which is then first rendered crystallizable. I am disposed to regard this crystalline substance as a combination with carbonic acid, and this view seems to derive confirmation not only from its formation in a current of carbonic acid and its spontaneous production in diseased liver, and from putrefaction, but also from the incapacity

of the solution to crystallize, after the dried or dissolved crystals have been placed under the air pump, and, finally from that decided development of carbonic acid which we perceive in the moist crystals in vacuo, and the obviously more abundant development of gas in vacuo, when acetic acid has been previously added to the solution. The globulin of the crystalline lens behaves precisely in the same manner, excepting that it is not crystallisable and does not require the previous application of oxygen for its separation by carbonic acid. In the chapter on the functions of the gastric juice will be found some facts well worthy of being remembered by physicians, especially in the treatment of Dyspepsia. Lehman finds "that the solvent power of the gastric secretion is suspended by boiling, by saturating the free acid, with an alkali, by tannin, alum, and most metallic salts, and is much impeded by addition of alkaline salts. Digestive power is restored in a certain degree by the repeated addition of the free acid, but too much free acid, without due dilution with water, entirely suspends digestive power. *Hydrochloric* and *Lactic acids* are the only acids which yield energetic, active digestive fluids with pepsin: sulphuric, nitric, lactic acids, yield with pepsin a digestive mixture of only slight power; while phosphoric acid, tartaric and succinic acids can in no degree replace the lactic or hydrochloric acid in the process of digestion. *Fats* when added in certain quantities to the gastric juice, promote the conversion of the protein compounds into pepsin." In the treatment of some rather intractable forms of dyspepsia characterized by feeling of weight and fullness after meals, eructations &c., and accompanied by great emaciation, we have derived great advantage, by ordering five minims of muriatic acid diluted with water taken just before each meal, and where there has been great debility, combining it with a few grains of the lactate of Iron, an artificial gastric juice has thus been found, and the patient in a comparative short period has been able to resume normal digestion.

We are compelled to pass over the highly important chapters devoted to the various organic constituents of the body, in order that we may examine more in detail the interesting facts connected with the functions of respiration. Many of Dr. Lehman's observations are new, and all are presented in such a clear and novel light as almost to deserve the name of new. We have the less reluctance in omitting any notice of the many interesting subjects in the preceding chapters, as we are sure that the work will soon be on the table of every physician. Dr. Lehman does not regard the lungs simply as organs of excretion, for not only do they excrete gaseous bodies, but they also absorb certain elastic

fluid substances. Modern investigations have placed beyond a doubt that there is an excretion of nitrogen, although in very small quantity. According to the observations of Reuling, "the exhaled air of every one contains ammonia: in health its quantity depends on the amount of ammonia in the inspired air, there being neither an absorption nor an elimination of ammonia by the pulmonary mucous membrane in healthy men." That nitrogen, however, is exhaled, and sometimes in considerable quantity, is proved by the experiments of Messrs. Allen and Pepys, who found that when guinea pigs were made to breathe in a mixture of oxygen and hydrogen, nitrogen was exhaled, and in a quantity exceeding in volume the whole body of the animal. Insects also exhale nitrogen, and fishes absorb it from the water in which they breathe, although they do not absorb hydrogen. (*Handbook of Physiology*, by Kirkes and Paget.)

According to Bousingault, two grains and a half of nitrogen are daily excreted from the skin and lungs, the quantity being increased or diminished according to the food taken.

It is a fact well worthy of notice, that the consumption of oxygen and consequent exhalation of carbonic acid is materially influenced by food. During fasting the consumption of oxygen is invariably less than when animals are well fed, and there exists an almost uniform ratio, for the most different animals, in regard to the composition of the air which is exhaled during the day; nay, from the experiments of Vierordt on himself, it appears that even the omission of a single meal alters the relation of the respiration very considerably. The excretion of carbonic acid is not only influenced by food, but by many substances which seem to act chiefly on the nervous system. Thus theine and alcohol materially diminish the amount of carbonic acid exhaled. In connection with this subject, we may notice the remarkable effects observed by Von Tschudi, as following the use of arsenic by the Styrian mountaineers. He says, arsenic is used by these people to give plumpness to the figure, and to improve the breathing, so that steep and continuous heights may be climbed without difficulty and exhaustion of breath. In commenting upon this, Professor Johnston believes that arsenic lessens the natural waste of the body by diminishing the quantity of carbonic acid discharged from the lungs in a given time. The consequence of this action upon the lungs will be, first, that less oxygen will require to be inhaled, and hence a greater ease in breathing under all circumstances; and secondly, that the fat of the food, which would otherwise be used up in supplying carbonic acid to be given off by the lungs, will be deposited

in the cellular tissue beneath the skin, and thus fatten the body. In endeavoring to render this singular circumstance useful in practice, we must confess to have failed, as after administering it to phthisical patients, with a view to diminish the rapidity of their respirations, we cannot recollect having observed any alteration or alleviation. In concluding our observations on Dr. Lehman's instructive work, we must mention that its value is much enhanced by the additions made by its accomplished translator, Dr. Day. We can confidently recommend it to our readers, as the best and most *eminently practical* work on the subject which has yet appeared.

I. L. CRAWCOUR, M.D.

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## Editorial and Miscellaneous.

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### TO OUR SUBSCRIBERS.

This number ends our second volume. We are pleased to announce to our patrons that our journal for the ensuing year will contain sixty-four pages in each number, instead of forty-eight as heretofore. Experience has proved that more space is required for valuable excerpts, as well as for more extended notices of books, &c. But, at the same time that we notice this enhancement in the value of our journal, we must inform our patrons that the subscription price will be increased to \$5 per annum. We find it utterly impossible to furnish a journal from the printing offices of this city at \$3 per annum, and cover the actual cost of the same. We know that our friends will not expect *more* of us than to work for *nothing*. Until our list of subscribers is largely increased, we cannot make one cent, but we are willing to contribute our labor if our friends will but pay the expense of publication; and although we must make the formal request of those who deem the \$5 too much, to return the first number of our new volume, still we do not *expect* such a reward at the hands of a single one of those for whom we have so long labored for the mere love of the enterprise.

To remind our subscribers of their pecuniary delinquencies is one of the most unpleasant duties which attends our editorial capacity; still we must here jog the memories of many of our country friends who are in arrears to us—some even for *two* years. If they could but behold more substantially than in the mind's eye the noble support of our city friends (not to omit an ever-generous few from the country), they would be at once stimulated to a spirit of emulation, if not positive rivalry. We have now not less than \$1,500 due us from the country, and yet the largest amount due from any one subscriber is \$6. Now, is there any comparison between our condition and that of our delinquent friends? We are largely "out of pocket" in paying the expenses of our journal; they have



our journal and their \$3 or \$6 too. "Look here, upon this picture, and on this!"

Our friends will at once accept our explanation of the delay in issuing this number. We have changed our publisher, and were under the necessity of sending north for a new fount of type, which has but very recently arrived. Henceforth promptness is our motto, and our publishers echo the sentiment.

For those who have heretofore advertised in our journal we have good news. By our new arrangements we are enabled to serve them at one-half the former rates. Our terms will be inserted in the first number of the new journal.

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SIMPSON'S OBSTETRIC WORKS. Priestley and Storer. First series. Lippincot & Co., Philadelphia.

We have before acknowledged the receipt of this most valuable volume, and although we promised to "notice" it, a strictly critical analysis of its 750 pages cannot be expected of us—we have not a sufficient number of pages to spare in our journal.

Dr. Simpson's work is eminently suited to the library of the practicing obstetrician: indeed it deserves a place on his table—at his very elbow; for he who once looks into it will soon perceive its great value as a book of reference on most of the important practical points in this branch of medicine. As a text-book for the student of medicine, however, it is by no means adapted, being on some subjects altogether too prolix, whilst on others it is strangely desultory. We have no reason to suppose, however, that the book was ever intended for students (unless it be, as the title intimates, for his own class), and shall only advise them to procure it after they have received their diplomas, so that they can profitably scan its pages during that interesting period in their professional existence, when they are "waiting for practice."

It is sufficient for our readers to know that this volume contains the views of one of the most prominent obstetricians of the age on as many as eighty-one different subjects, the arrangement of which is most highly creditable to the industry and good taste of the editors.

Dr. Simpson has shown himself not only an original thinker, but a man possessed of more than an ordinary share of independence. In his articles "On Turning, as an alternative," for craniotomy and the long forceps, in deformity of the brim of the pelvis, &c., he lays aside all the preconceived ideas of the authors as to both the theory and practice in such cases, and not only boldly asserts for his "alternative" the highest claims to consideration, but surrounds himself with a bulwark of facts which seems to us totally impregnable.

More than this, Dr. Simpson has happily availed himself of a means of defence seldom so successfully taken advantage of; the arrows from the quivers of his enemies, which have fallen harmless at his feet, he returns with unerring aim and deadly effect; he takes their *own statistics*, their *own notes*, and makes out a case far stronger for himself than they could ever have imagined theirs.

It has ever appeared to us strange indeed that the practice of almost indefinitely awaiting the efforts of nature to relieve a complicated obstetric case should have so generally obtained throughout the world. During our student's life we were thoretically, and in some degree practically, opposed to the teachings

of text books and lecturers, and our opposition has only been strengthened by every additional item of experience which we have acquired. Most cordially then, do we endorse the four prominent propositions (demonstrated most clearly) advanced by Dr. Simpson.

1st., "*The mortality attendant on parturition increases in a ratio progressive with the increased duration of the labor.*"

2nd., "*The infantile mortality attendant on parturition increases in a ratio progressive with the increased duration of the labor.*"

3d., "*The liability to most of the morbid complications connected with labor increases in proportion as the labor is increased in its duration.*"

4th., "*The operation is dangerous and fatal in proportion to the length of labor allowed to elapse before the artificial delivery is practised.*"

It is high time that our teachers should lay aside the antiquated notion of nature's performing mechanical impossibilities: if there are cases wherein the interference of art is palpably indicated, and if the obstetric instrument is safe in skillful hands, in the name of common sense let us resort to our remedial measures while the chances of doing good are mostly in our favor. The idea seems to be, that skillful obstetricians being comparatively few, it is better to teach the inexperienced young man to expect more of nature than she may really be able to accomplish, and that his instruments are only to be used as a dernier resort—in fact, that assistance from him is an act of desperation only warranted by the impending death of both mother and child. Better far would it be to teach these gentlemen to be skillful, to demonstrate to them the true nature of the instruments, and let them go forth prepared to use them with benefit to their unfortunate patients. Thus, and thus only, will an unmerited opprobrium be lifted from the most important branch of the obstetric art.

While we are most liberal in our praise of this work, however, we regret to have to say that there is one very objectionable feature attached to it. We allude to such of the American editor's prefatory remarks as refer to Dr. Robert Lee, of London. "Comparisons are odorous," says Mrs. Partington, we believe; and in no instance have we seen the adage more strongly exemplified than in this. The American editor is right when he supposes Dr. Simpson will not approve of his remarks—"that it will give him pain"—and we think, moreover, that he presumes too much when he undertakes either to decide, in the preface of Dr. Simpson's work, on the issues between these eminent gentlemen, or on their relative merits and professional standing. We can assure the editor, too, that all medical men, either here or abroad, do not hold the same opinion regarding the subject of Dr. Robert Lee's system of uterine nerves and the royal medal. But even if the profession did wish to have a single man decide these questions, they would hardly apply for an impartial judgment at the hands of the editor, who says of Dr. Simpson, "I learned to love him as my father; he treated me as a son. I am proud to call him *master*."

Oh! how we Americans (not omitting "American editors") are given to toadyism! If a "foreigner" wishes to own us, he has only to allow us the privilege of eating the crumbs from his table, and he is "master," and we his humble servants.

TO THE EDITORS OF EXCHANGE JOURNALS.—In preparing our exchanges for the binder, the following Nos. are “found missing,” and we must ask the favor of our brother editors to furnish us the same if they can possibly spare them.

“*Virginia Medical and Surg. Journal.*”—Jan., Feb., June, Sept., Oct., Nov., and Dec. Nos. 1854—May., Aug., Nov. 1855 Nos.

“*Charleston Med. Journal and Review.*”—Sept. and Nov. 1855 Nos.

“*Stethoscope.*”—Jan., Feb., March, April, May, Sept., Oct., and Nov., 1854 Nos.—April, Aug., Nov., 1855 Nos.

“*Ameri. Journal Med. Sciences.*”—Jan., April, and Oct., 1854.

“*Med. News and Library.*”—Jan., Feb., March, April, May, June, July, Aug., Sept., Nos. 1854—Feb., May, June, Aug., Sept., Oct., Nov. and Dec.’ Nos. 1855.

“*New Jersey Med. Reporter.*”—Jan., Feb., March, April, July, Aug., Oct., Nos. 1854—Feb., March, 1855 Nos.

“*Nashville Journal of Med. and Surg.*”—Nos. 1, 2 and 3, of vol. 7—No. 1, of vol. 8—Nos. 2, 5, 6, vol. 10.

“*New Hampshire Journal of Med.*”—Jan., Feb., March, Aug., Sept., Nos. 1854—April, 1855 Nos.

“*Med. Examiner.*”—Nos. 1, 2, 3, 4, 5, 7, 9 of vol. 10, new series—No. 8, 11 and 12 of vol. 11,

“*New York Journal of Med.*”—May, No. 1855

“*Buffalo Med. Journal.*”—Aug., and Dec., 1855.

“*North-Western Med. and Surg. Journal.*”—Jan., Feb., March, April, Aug. and Dec. 1855.

“*Amer. Med. Gazette.*”—Nos. 1, 2, 4, 5, 6, 7, 8, 10, 11 and 12 of vol. 6.

“*Southern Med. and Surg. Journal.*”—Jan., Feb., May, Aug., Oct., Nov., Dec. 1855.

“*Amer. Med. Monthly.*”—March, April, June, Oct., Nov. 1855

#### NOTICES OF BOOKS AND PAMPHLETS RECEIVED.

“A Brief History of the Origin, Progress and Extension of the Yellow Fever in Memphis, Tenn., 1855.” By L. Shanks, M.D., Professor, &c.

“The Practitioner’s Pharmacopæia and Universal Formulary.” Foote. S. S. & W. Wood, New York, through Thos. L. White, Canal street.

“Anatomical Remembrancer,” second American edition. Isaacs. S. S. & W. Wood, New York. T. L. White, Canal street.

“Reese’s Medical Lexicon.” S. S. & W. Wood, New York. T. L. White, Canal street.

“Vidal on Venereal.” S. S. & W. Wood, New York. T. L. White, Canal st.

“Scenes in the Practice of a New York Surgeon.” Dixon. De Witt & Davenport. We have only had time to give this little book a cursory reading. It appears to be both amusing and instructive.

NEW MEDICAL JOURNAL.—We are in receipt of the first number of the “Cincinnati Medical Observer,” a very neat monthly journal, edited by Profs. Mendenhall and Murphy, of Miami Medical College, and Edward B. Stevens, M.D. The editors avow their determination to be “defenders and promoters of scientific

medicine in its broadest, progressive and orthodox sense." Under such circumstances we welcome them into our ranks, and shall be pleased to place them on our exchange list.

We are also in receipt of the January, 1856, number of the "Medical Reporter," a quarterly journal, published under the direction of the Chester and Delaware County Medical Societies. We place the same on our exchange list.

*List of Mortality for Four Weeks, ending Jan. 31, 1856.*

	1st week.	2d week.	3d week.	4th week.	Total
Total number of Deaths.....	88	91	108	78	351
Males.....	49	60	50	41	190
Females.....	28	35	42	32	137
Sex not stated.....	10	6	16	5	38
Whites.....	64	74	82	58	268
Blacks.....	14	11	22	11	56
Mullattos.....	6	6	9	9	30
Color not stated.....	4	0	6	0	9
Native Americans.....	10	16	18	14	57
Northern States.....	2	2	3	3	10
Western States.....	0	0	2	3	5
Southern States.....	8	14	12	8	42
Foreigners.....	29	27	31	23	111
English.....	2	0	1	1	4
Irish.....	12	16	10	8	46
French.....	4	3	3	3	13
German.....	5	5	10	4	24
Place of birth not stated.....	49	47	60	41	197
Age not stated.....	12	14	31	13	60
Under one month old.....	12	12	10	9	43
From one to five years.....	11	16	13	15	54
From five to ten years.....	3	1	1	2	7
From ten to twenty years.....	4	1	2	1	8
From twenty to thirty years.....	9	13	17	12	51
From thirty to forty years.....	9	12	16	11	48
From forty to fifty years.....	10	10	9	5	34
From fifty to sixty years.....	9	5	7	5	26
From sixty to seventy years.....	4	2	2	2	10
From seventy to eighty years.....	3	1	6	2	15
From eighty to ninety years.....	2	4	2	0	9
Over ninety years.....	0	1	2	1	4
Yellow Fever.....	0	0	0	0	0
Other Diseases.....	72	79	91	73	315
Typhoid Fever.....	0	2	4	0	5
Cholera.....	0	1	1	0	2
Intemperance.....	0	2	0	0	2
Accidental.....	0	0	0	0	0
Still Born.....	8	3	12	4	27
Disease not stated.....	8	4	0	1	13

*Charity Hospital Report for the month of January, 1856.*

	Males.	Females.	Total.
Admitted.....	702	149	851
Discharged.....	568	121	689
Died.....	85	17	102
Report of Births.....	6	5	11



ERRATA.—In our Jan. No., at page 500, 22nd line from the top, read, will not be, instead of “will be.” At page 516, first line of third paragraph, instead of “treat,” read think, Also, at same page, fifth line in second paragraph, there should be a comma after the word “having.” At page 518, fifth line of fifth paragraph, instead of “note,” read noted. At page 519, fifth line in last paragraph, read consumption instead of “consumptive.”

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Excerpta.

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CLINICAL LECTURES ON PURULENT URINE AS SYMPTOMATIC OF DISEASES OF THE KIDNEY.—By W. R. Basham, M.D., Physician to the Westminster Hospital, and Lecturer on the Practice of Medicine.—Notwithstanding that diseases of the kidneys have been studied with results as advantageous to correct diagnosis as they have been beneficial to the general principles of treatment, there are, nevertheless, morbid conditions of these organs, which still, and perhaps ever must, except under very peculiar circumstances, present difficulties and obstacles to a correct diagnosis. Tubercle of the kidney is a disease of this character; it is of comparative unfrequency, and if the record of such a case does not assist in clearing away the obstacles to a correct estimate of the presence of this deposit in the kidney, it will, however, exhibit the group of symptoms, as well as the complications which may arise and become associated with tubercle in these organs. The following case will demonstrate that while the existence of pyelitis, probably from calculus in the pelvis of the kidney, as well as the sacculated and enlarged condition of this right kidney, was clearly made out during life, yet on the other hand there was nothing in the urinary secretion or other symptoms that could suffice for the suspicion of the presence of tubercle. The amorphous organic granular matter, insoluble in hydrochloric or acetic acids, and associated with pus in the urine, which has been supposed characteristic of softening tubercle of the kidney, was in this case so masked by the presence of amorphous granular earthy (phosphatic) matter, but readily soluble in these acids, that tubercle was neither suspected nor recognized. And even after death, when the nature of the purulent contents of the kidney was known, and submitted to the microscope, the presence of this organic granular matter was very doubtful.

J. W——, aged twenty-nine, a carpenter, was transferred from the care of Mr. Charles Guthrie, and admitted into Burdett ward on the 18th of May, 1855. He came under the care of that gentleman, suffering from irritable bladder, and doubtful symptoms of calculus. He was examined by Mr. C. Guthrie, Mr. Holt and Mr. Brooke, and they were unanimously of opinion that there was no stone in the bladder, nor stricture, nor disease of the prostate. While under surgical treatment, samples of his urine were twice sent to me for examination. On the first occasion, the urine was faintly acid, slightly albuminous, natural in color, and depositing, when set at rest, a faint flocculent cloud; there were also small membranous shreds, one or two of which contained a minute coagula of blood. Under the microscope there were seen the large pavement epithelial cells in abundance; many blood corpuscles; some fibrinous coagula, quite amorphous, but stained of a reddish yellow; and a few of the smaller, spherical, glandular epithelium. The bottom of the glass vessel in which the urine had remained, contained a few membranous shreds, which felt gritty, and which readily dissolved in dilute hydrochloric acid. The patient described his symptoms as of about some seven weeks' duration: he could not account for their origin, but described them as commencing with severe lumbar pain, of a sharp, pungent character, followed by frequent desire to pass urine, and inability to retain but a

small quantity. This was followed by severe paroxysms of pain, referred to the perineum and canal of the urethra. The irritability of the bladder increased; he passed urine every hour, day and night; he had never passed blood, but he had seen very minute threads of coagulated blood. The house-surgeon stated that he had once noticed the urine of a dark chocolate color; and a few days before he was transferred, it became opaque and milky. As the pain became more urgent and distressing, his bodily strength failed, and he rapidly lost flesh. While under Mr. C. Guthrie's care, he had been cupped in perineo; he had taken, also, diuretics, with liquor potassæ; opium and ether had also been given, but from none of these remedies did he experience any relief. On being received into Burdett ward, the following were the chief symptoms recorded in the ward-book: there was great physical exhaustion and debility, much emaciation, and a careworn, anxious expression of countenance. The pulse was small and weak, the respirations natural, the tongue slightly coated, torpid bowels, deficient appetite, craving thirst, and profuse and exhausting perspirations. The patient complained of urgent pain, referred to the hypogastric region of the abdomen, and accompanied by darting, stabbing paroxysms in the perineum. This pain, the patient states, is temporarily relieved by micturition, that for a few minutes afterwards he is comparatively easy, but that the uneasy sensation then commences, and continues augmenting in intensity till the distress becomes most urgent, and is again temporarily suspended by voiding the small amount of fluid collected in the bladder, which seldom exceeds one or two ounces. The pain is not increased by walking or exercise, and he has never known the urine to stop suddenly, and he is quite clear that the only interval of ease that he experiences is immediately after micturition. He has for many weeks suffered from a constant fixed pain in the small of the back, to the right of the spine, and referred to the posterior crest of the ilium. There is a greater degree of fulness in the right lumbar region than in the left, and pressure made on the former, by grasping this region with the right hand, the thumb resting on the lumbar muscles, and making pressure on the abdominal wall with the fingers, excites an increased amount of pain. An obscure sense of limited fluctuation is also elicited out of the same region. These conditions are absent on the left side.

The urine passed last night is milky and opaque, which characters the patient now states he has noticed during the last week. On being set aside it separates into two portions: an upper, slightly clouded, but otherwise naturally-looking urinary fluid; and a lower, distinctly separated precipitate of yellow pus corpuscles. The upper portion is not ropy, but pours off naturally, is faintly acid, and a copious precipitate of albumen is obtained by heat and nitric acid. Examined by the microscope, the lower part is composed entirely of pus corpuscles, with granular walls, and with addition of acetic acid these nuclei become distinctly visible. Some granular matter observed in another sample was readily dissolved by dilute hydrochloric acid. The supernatant portion before the glass exhibited large spheroidal epithelial cells, with many scattered pus corpuscles.

The opinion expressed after these facts had been elicited was, that the patient was suffering from pyelitis of the right kidney, due in all probability to the impaction of the calculus in the ureters or its lodgment in the head of that outlet from the pelvis; that the right kidney was enlarged and sacculated from this cause; and that the irritation of this concretion had set up an inflammation of the mucous membrane lining the pelvis and extending probably to the calyces; and that the products of this inflammation (only partially retained by the body lodged in the head of the ureter, for the regular shape of these concretions does not completely close the canal,) on the one hand, passed as it were, stillatim, into the bladder: while, on the other, it was continually exercising a dilating or expanding pressure from within outwards, causing the kidney to become distended, and ultimately sacculated with the entire loss of its tubular structure.

The *uvæ ursi* was first administered, with a grain of morphia, night and morning; but, in a few days, the morphia appearing to fail in its anodyne

effects, Battley's solution was substituted, and for a short time with apparent relief; on the 29th of May the ward-book states that the pain was neither so urgent or distressing, nor the desire to pass urine so frequent. The quantity passed each time, however, did not exceed two ounces; and the whole quantity during the twenty-four hours at this time amounted to not more than sixteen ounces. The character of the urine continued the same; a dense, well-defined purulent deposit, and a supernatant, now perfectly clear portion, faintly acid and albuminous. Emaciation continues; there were profuse perspirations; much thirst, and the tongue becoming brown and dry; pulse very small and weak.

On the 2d of June, he took quinine and sulphuric acid, the anodyne being continued; the former of these remedies seemed to check the perspiration.

On the 12th of June, there was some apparent improvement in the general condition of the patient. The thirst was less, the perspiration less, and the tongue, although red, was moist, and the patient had slept better. The urine passed in the latter part of the day emitted a very offensive odor, and was for the first time alkaline, and contained a much larger proportion of purulent deposit; the upper portion was, however, clear, and not repy, and abundantly albuminous. There followed so much exhaustion on the next day that he was placed on ten ounces of port wine. Micturition became more difficult than at any previous period of the disease. The opium continued to procure slight mitigation of the symptoms.

On the 14th, there was much exhaustion, pallor of the countenance, and the urine passed involuntarily from him; the quantity could not be estimated, nor the character ascertained. On the evening of this day the nurse reported that he was suffering from loose watery diarrhoea.

On the 16th, the ward-book stated that although much more exhausted, yet the patient stated that he felt easier, and that the pain referred always to the neck of the bladder and perineum had subsided. An opinion was expressed that in all probability a communication had become somewhere established between the enlarged and sacculated kidney and the intestines, by which the contents of the renal organ had been discharged, and relief for a time obtained. The condition of the patient was such that no examination of the loins and abdomen could be efficiently made. He lingered for two more days. The watery discharge from the bowels continued, and a turbid fluid escaped now and then in small quantities from the urinary passages. He died on the 18th of June.

*Post-mortem examination eighteen hours after death.*—The body was much emaciated. On opening the chest, the lungs only partially collapsed; a few scattered bands of old organised lymph united the pleuræ in both cavities. Both lungs were the seat of tubercular deposit in the form of small grey granules, not exceeding a pin's head in size, nowhere in a stage of softening, nor were they in any spot aggregated together, but were diffuse, distinct, and surrounded by elastic, permeable lung substance; the bronchial mucous membrane was natural in appearance throughout. The heart was soft and flaccid, with two white opalescent patches on its surface, the interior cavities and the valves were free from any morbid appearance. The cavity of the abdomen: the peritoneal surface of the intestines presented no inflammatory appearance, except at one spot, where a fold of small intestine, lying in contact with the fundus of the bladder, was covered with some recently effused lymph, and surrounded with a margin of injected vessels. The colon was traced from the cæcum, and on raising it the right kidney was observed much enlarged, of a somewhat quadrangular shape, apparently sacculated, and fluctuating distinctly to the touch, from contained fluid. The large intestine was traced to the rectum, but no morbid condition was noticed; but the rectum, at a spot corresponding to the posterior and inferior third of the bladder, and a little to the left of the median line was firmly united, and a fistulous communication at this point had become established between the bladder and the intestine, and by this channel the contents of the bladder had, since the commencement of the fluid dejections, been voided by the rectum. The mucous membrane of the bladder was of a dark slate colour, the upper half



presenting the usual corrugations, but below the fistulous opening, and surrounding it, were numerous granular deposits, presenting the character of tubercle; the prostrate was also the seat of a granular deposit. The effused lymph thrown out on one fold of the small intestines lying in proximity to the bladder was not more than a finger's breath in extent, and had been developed by contiguity to the fistulous opening between the bladder and rectum. Within the bladder, the edges of the fistulous passage had a ragged and gangrenous appearance, and immediately beneath, and filling the portion of the bladder inferior to the fistula was a mass of fibrinous matter, gelatinous in character, and adherent, though not firmly, to the mucous membrane; there was much amorphous and gritty matter diffused through it, and in the most inferior part fragments of earthy, gritty matter, composed of triple phosphate and urate of ammonia. The two kidneys differed very materially from each other. The right was much enlarged: it distinctly fluctuated: and was lobulated. The left, though somewhat larger than natural, preserved its form, and presented no evidence of disease. The capsule of the right kidney was firm and dense, and was with some difficulty dissected from the cortical surface. When this was accomplished, the surface was found studded with white tubercular spots, each surrounded by a faint-red areola. On a section being made, a considerable quantity, about ten ounces, of a purulent fluid, of a yellow colour, and creamy consistence, escaped, and displayed the interior of the kidney as a large sac, each of the calyces representing a small cup-shaped cavity, constituting the walls of the dilated kidney. These smaller cavities, as well as the pelvis of the kidney, were covered with a rough ragged fibrinous deposit. The ureter was much thickened, and throughout its extent was cord-like, its canal much narrowed, and its lining membrane of a slaty colour. There was not complete occlusion of its passage, for a small probe could be passed into the pelvis of the kidney, and, before the sac was laid open, the purulent fluid from the interior could be expressed from it. All trace of the tubular portion of the kidney had disappeared. The cortical part, when examined under the microscope, exhibited only a fibrous material, with diffused granular and fatty matter. The urethra presented no evidence of disease. The mesenteric glands were enlarged, and were the seat of tubercle. The liver was natural in size and weight, but somewhat soft and greasy, and the hepatic cells were more loaded with fat than in health. A few scattered tubercles were present in the spleen.

The significant symptoms at the period this case first came under observation may be conveniently placed in three groups—

I.—The sensations of the patient in reference to the function of micturition, its frequency, and the subsidence of the pain after voiding urine.

II.—The proofs obtained by examination of the bladder and urinary passages that there was neither stricture, calculus in the bladder, nor prostatic disease.

III.—The microscope and chemical examination of the urine, and the physical examination of the regions of the kidneys.

Frequency of micturition arising from, or associated with, pain at the neck of the bladder, or perinæum, or extremity of the glans penis, is a condition common to many disorders of the urinary organs, and is present in a greater or less degree in stricture of the urethra, calculus in the bladder, disease of the prostate, general irritability of the bladder, and in some special forms of disease of the kidney. A surgical examination of the canal of the urethra, or an exploration of the bladder, may be necessary to determine the presence of one or other of the first-named conditions. But the symptom of most practical importance in connexion with this part of the subject is the relation which the pain and uneasiness experienced by the patient bears to the function of micturition or the act of emptying the bladder. While painful and frequent micturition is common to all the above-named disorders, yet the period during which the greatest aggravation or the greatest relief occurs, materially differs in each. Thus, in stricture the pain and distress is greatest during the effort, or during the act of voiding the urine. In stone in the bladder the pain is greatest after micturition, or when the bladder is empty. In disease of the prostate, the pain



is continuous, and not relieved by micturition. While in those cases of pyelitis particularly calculous pyelitis in which irritability of the bladder exists—for it is not present in all cases—the pain, distress, and uneasiness is quickly and immediately relieved, by micturition, however small the quantity passed; and in this respect these cases exhibit conditions and symptoms analogous to what is observed in ordinary irritability of the bladder and urinary organs, where there is pain and frequency of micturition; but the morbid conditions are readily distinguishable by a microscopic and chemical examination of the urine. In the case under consideration this symptom was very expressive; the patient spoke distinctly of the pain and distress acquiring intensity as the urine accumulated, though only to the extent of an ounce or two; that he experienced no difficulty in passing urine, and that the relief after micturition was complete. At the time he first came under notice this period of relief extended to an hour or two; latterly the interval of ease became more and more limited, and towards the last, although the patient made no special complaint, yet it can hardly be doubted that the suffering was continuous, and allayed only by the anodyne influence of opium. This aggravation of the symptom must be attributed to the tubercular deposit in the prostate and the bladder.

II.—Before the patient left the Surgeon's ward, he had been carefully examined by Dr. Holt, Mr. Charles Guthrie, and Mr. Brooke, who were unanimous in their opinion that there was neither stricture, calculus, nor prostatic disease.

III.—*The State of the Urine.*—The urine at the earliest period at which it was submitted to me for examination, was faintly albuminous, and contained numerous shreds, or fibrinous flocculi, some of which contained a minute clot of blood, visible to the naked eye; latterly some of these fibrinous coagula contained gritty matter. This flocculent deposit presented, under the microscope, the characters of coagulated fibrine, stained with coloring matter; there were also scattered blood corpuscles, and numerous large-sized spheroidal epithelial cells, some of which by apposition, presented a polygonal aspect, having the usual character of epithelium from the pelvis of the kidney. The urine soon after became milky, and deposited a precipitate of pus corpuscles; the upper portion of the urine remaining clear and natural in color, but highly albuminous. The pain, on deep pressure being made in the right lumbar region, and the greater fulness and obscure fluctuation of this part, completed the evidence in favor of the diagnosis that was formed. The presence of tubercle was not suspected; and it may be interesting at this point to inquire whether there are any and what means of distinguishing tubercle of the kidney during life? and another question also may be raised here, although perhaps it may not be capable of easy solution—viz., what relation did the tubercular deposit in the kidney bear to the earthy phosphatic concretion found in the pelvis of the kidney, and partially obstructing the ureter?

Let us examine these two questions separately. Are there any and what means of determining the presence of tubercle in the kidney during life? This question, I fear, at present must be answered in the negative. Rayer says, "Je ne connais aucun caractère dans la sécrétion urinaire, ni aucun autre moyen, à l'aide duquel on puisse diagnostiquer le dépôt de la matière tuberculeuse dans les substances rénales." He mentions circumstances under which it may be less difficult to determine the presence of tubercle; but supposing the tubercular deposit not to extend beyond the kidney, and not to affect the bladder or urinary passages, there is no condition of the urine, or other symptoms, by which tubercle of these organs can be correctly determined. It has been thought that the presence of an amorphous granular matter in the urine insoluble in dilute hydrochloric or acetic acids, accompanying the pus cells, may be accepted as a distinguishing feature of a tuberculous kidney in a state of ulceration. But this is a point of some difficulty to determine. In this case there was at times much amorphous granular matter seen under the microscope, accompanying the pus corpuscles, but this was easily dissolved in either of the above-named acids; and as it was found associated with the gritty matter imbedded in the fibrinous coagula, it was at once recognized as amorphous earthy phosphate. Where the opposite kidney is

not diseased, urate of ammonia is oftentimes present, and would not dissolve in these acids, and might, to a superficial observer, be mistaken for organic amorphous matter: it would be readily removed by distilled water at a temperature of 90°. But these do not constitute all the difficulties in the way of correctly estimating this amorphous granular matter. Whatever causes the disintegration of any of the pus cells would diffuse over the field an amorphous matter which would possess all the requisite chemical qualities of insolubility in acids, and yet it would not be necessarily characteristic of tubercle. I conclude, then, in the present state of our knowledge, there is no condition of the urine which can be accepted as a diagnostic sign of tubercular ulceration of the kidney.

The second question—What relation did the tubercular deposit in the kidney bear to the earthy phosphatic concretion found in the pelvis of the kidney, and partially obstructing the ureter?—is one involving a conjecture as to the priority of either of these conditions: Did the earthy concretion first impede the functions of the organ, and did the consecutive pyelitis induce the deposit of tubercle in the kidney? or was the tubercular disease, by embarrassing the excretory function of the kidney, the cause of such changes in the urine, before it could find its way to the bladder, that the formation of the earthy phosphates, in the pelvis of the kidney, was the result? Guided by the earlier symptoms of the case, the inference must be that calculous pyelitis existed before tubercular ulceration was established; but whether before tubercle was deposited cannot be determined. The symptoms of calculous pyelitis were sufficiently marked, even before any notable amount of pus appeared in the urine. Lumbar pain, difficult, painful and scanty micturition, minute blood clots with membranous flocculi in the urine, resolved by the microscope into fibrinous shreds, blood corpuscles, epithelial cells from the pelvis of the kidney, and minute crystals of phosphatic concretions, forcibly indicated this disease; and it was not till some weeks afterwards, that the large and abundant deposit of pus in the urine indicated that a process of ulceration was established. The post-mortem examination proved that this was connected with the tubercular deposit. The primary source of irritation, then, would appear to have been the calculous concretion which partially obstructed the mouth of the ureter. Under ordinary circumstances, with the other kidney unaffected, this condition might have continued for years. I have had one case of the kind under observation for the last nine years; and I have a preparation of a sacculated kidney with the calculus impacted in the head of the ureter, the duration of the symptoms during life being spread over an interval of nearly seventeen years; so that the rapid and fatal issue of this case must be attributed to the process of ulceration set up in the substance of the kidney, occurring in an individual in whom there existed, as the result proved, an overwhelming tubercular diathesis. This tubercular dyscrasia was forcibly exemplified by the wide sphere over which this product was deposited, existing in a crude state in the lungs, spleen, mesenteric glands, and prostate, and in an active state of ulceration in the kidney and bladder.

Rayer (*Traité des Maladies des Reins*, vol. iii., p. 643 *et seq.*) has recorded several cases of tubercular deposit in the bladder accompanying tubercle of the kidney; but the peculiar feature of this present case consists, not so much in the presence of tubercle in the bladder, or its wide diffusion in other organs of the body, as in the tubercular ulceration in the bladder, and the fistulous communication which was established between the bladder and the intestine. Cases in which fistulous communication has taken place between sacculated kidneys with obstructed ureters and the large or small intestines, although rare, are not without example. Rayer mentions several. Two such cases have occurred under my own observation. In one remarkable case, the particulars of which I propose to communicate hereafter, a large encysted and sacculated right kidney opened a passage for its contents into the contiguous ascending colon, and a temporary cure was effected by what was supposed to be a violent diarrhoea. But I am unacquainted with any analogous case of a fistulous opening between the bladder and rectum caused by tubercular ulceration in the mucous membrane of the former.

Upon a careful consideration of the contents and condition of the bladder, I think it will not be difficult, to a certain extent, to account for the amount of obstruction to the escape of the contents of the bladder through the urethra, which have pre-existed and operated as the direct cause of this fistulous ulceration. Up to the last week of his life, the patient never experienced any difficulty in passing urine; the frequency of micturition was not accompanied by any condition that indicated obstruction or difficulty; and at the post-mortem examination, although tubercles were observed in the prostate, yet these were granular and inactive, and the gland was not enlarged to an extent sufficient to form any impediment to the free passage of the contents of the bladder. Within six days of the patient's death, it was observed that he no longer passed his urine voluntarily, and it was reported in the ward-book, that what little was passed seemed to dribble away from the urethra; on the next day, the diarrhoea, as it was reported, suddenly set in, and it was at once conjectured that a communication had somewhere been effected between the kidney and intestines: but the opening had taken place between the bladder and rectum, and not between an obstructed and sacculated kidney. So long as the urinary passages are free, such ulcerative communications are little likely to be formed. There was no stricture of the urethra; there was no prostatic disease sufficient to cause retention of the contents of the bladder. How, then, did the bladder fill, and through what cause were its contents forced to an outlet by the lower bowel? At the bottom of the bladder, adherent to the mucous surface, and filling the space between the ureter of the right side and the opening of the urethra, was found a gelatinous coagulum, of a true jelly-like consistence and color, and in the lower part of which some earthy gritty matter was found. This gelatinous exudation constituted, I believe, the proximate cause of the obstruction to the escape of the contents of the bladder, and the consequent retention led to the fistulous communication with the rectum. I think this opinion is corroborated by the observations of Sir Benjamin Brodie. In his work on "Diseases of the Urinary Organs," p. 134, he mentions a case in which a mass of lymph, of the consistence and appearance of jelly, was found in the urine. The patient suffered from frequent micturition, and had a deposit of pus in his urine, and on the occasion when this gelatinous matter passed, he suffered from symptoms which were thought to indicate the passage of a renal calculus. The pain suddenly ceased with the escape of this jelly-like fibrine, but the patient subsequently died, and Sir Benjamin Brodie concluded that the immediate cause of this patient's death had been retention of one of these fibrinous masses.

Nothing can be calculated effectually to obstruct the escape of urine by the natural passage than one of these gelatinous masses filling the lower part of the bladder. Tubercle, already deposited in the tissues of the bladder, offered a condition in which the ulcerative process might most readily commence, and a fistulous opening be quickly established. An exudation of plastic lymph on the surface of the bladder, as well as on the intestine, surrounding the ulceration, effectually preserved the peritoneal cavity from any infiltration of the contents of the bladder.

In these cases, we cannot permit ourselves to hope that the result of treatment can be other than palliative. When the suppurative process in the kidney arises from tubercular ulceration, its exhausting effects rapidly reduce the vital powers, and neither nutritive substances, cod-liver oil, nor other medicinal agents can be expected to arrest the fatal process. Of the remedial means most productive of benefit, opium, in the form of Battley's solution, or the preparation of morphia, yield the most satisfactory results.—*Lancet*.

LODGMET OF PART OF A RIFLE-BALL IN THE BRAIN, WITHOUT THE USUAL EVIDENCE OF A HOLE IN THE CRANIUM.—Private William Doyle, 19th Regiment, aged 19 years, was wounded in the head by a rifle-ball, in the advanced trenches of the right attack, on the 13th of August, 1855. The scalp and pericranium were cut about two inches, and a portion of the cranium, about an inch in length, and half an inch in breadth, a little in advance of the posterior and superior angle of the right parietal bone, and close to the sagittal suture, was



depressed. According to statement, the man was rendered perfectly senseless and motionless from the instant of being struck by the bullet, so much so, that at first he was supposed to be dead.

On being conveyed to camp, he presented the usual symptoms indicating compression, pupils dilated and fixed, warm surface, unconsciousness, complete paralysis, &c. On examination of the depressed portion of bone, no opening whatever could be felt, the edges of the sunk bone, and the bone adjoining, were in contact, and it was presumed to be an ordinary case of fracture, with depression. Some very minute portions of cerebral substance were observed to be mixed with the clot about the wound, such as might be squeezed through a fissure. Trephining being determined on, was performed at once, and the depressed bone raised without difficulty. No relief whatever followed. The dura mater bulged slightly upward in the opening. On passing the finger over its surface, a little beyond the space exposed by the trephine, a defined cut edge was felt, a little more than an inch in advance of the site of the depressed piece of bone, being the boundary of an opening into the cerebral substance.

Three hours after arrival in camp, the patient sank. On post-mortem examination, a wedge-like section of the ball was found to have entered, and penetrated the cerebral substance, and was discovered in the anterior lobe on the right side, just above the orbital plate. It had not completely penetrated the dura mater, but was lying just within the membrane covering the lobe. The ball—a conical rifle-ball, with three cannellures—was cut smoothly from apex to base, as if by a sharp knife. This must have been done by the edge of the broken bone, above the opening made in the parietal bone, one-half of the ball flying off, the other entering the skull. On close examination, several minute points of lead were found to be imbedded along the fissured margin alluded to. The depressed piece of bone, directly after the section of the ball entered, must have sprung up by its own resiliency, or been forced up by sudden pressure from within, so that no evidence of an aperture, but merely a depression and fissure remained. The inner table was separated, and nearly detached, from a space rather more extensive than that of the depressed part of the outer table. The superior longitudinal sinus was wounded by the sharp edge of the broken inner table, and a considerable quantity of blood extravasated upon the surface of the brain. The portion of bone implicated in this injury has been preserved. Weight of piece of bullet, 5 drs. 5 grs.—*Lancet*.

**REMARKABLE CASE OF SUBCLAVIAN ANEURISM; NEW METHOD OF TREATMENT; RECOVERY.**—Mr. Fergusson presented to his class on the 4th of August, a most interesting case—one of a series, as we subsequently learned—where a very remarkable cure has been effected in well marked subclavian aneurism, by a new and specific method of manipulation which he has adopted. We may state here that we saw the case about a year and a half ago also, when the man was previously under treatment. Some short period before that time Mr. Fergusson conceived the plan of stopping the circulation in the aneurism by pressing the sides of the aneurismal sac together, with their intervening fibrinous deposit, and in this case, from the phenomena attending the manipulation, there appear to us very little doubt that the object held in view by Mr. Fergusson had been attained—viz: the clots of fibrin in layers in the aneurismal sac had been displaced, and, spreading from the subclavian into the axillary and brachial, a new sort of Brasdor's operation, at the distal side of the subclavian had been the result. In other words, we believe Mr. Fergusson here, without ligature, had attained all the advantages of the last-named operative proceeding; for not only had a blocking-up of the axillary and brachial been followed by a partial stoppage of the current through all the enlarged aneurism of the subclavian, but even with very marked, but not so satisfactory, results as regarded the pulse in the radial at the wrist, which became completely stopped for a time, with symptoms of paralysis in the arm, all resulting from the displacement of the fibrinous clots.

The aneurism in the present case was situated in the subclavian; in the usual



site of subclavian aneurism—namely between the scaleni muscles, and to us seemed almost to invite some modification of the Dublin surgeons' plan by compression on the first rib. The plan by compression, we need hardly observe, is in general applied to the artery above the aneurism, between the latter and the heart. Crampton, however, in 1816, showed that the obliteration of an artery can be effected without rupture or ligature of its coats, as generally conceived, simply by this blocking-up process. The early volumes of the *Lancet* contains cases also cured by Brasdor's operation; it seems however more applicable to carotid than subclavian aneurism.

Mr. Fergusson related to his class on the 11th, at some length, the details of a previous case of subclavian aneurism of the same character as the present, in which his ideas on this subject were first matured. In both cases the method of cure by deligation at the tracheal side of the scaleni, as well as Brasdor's operation at the distal end of the aneurism, were inadmissible; yet it was gratifying to find the present plan, by firm pressure of the thumb on the aneurism, so as to displace some of the fibrinous clots, followed up by local pressure, succeeded in obtaining most striking and in many respects curious but satisfactory results. Intimately associated as the subclavian is at the right side with the vertebrals and carotid, the method of displacing fibrinous coagula is not without danger. A patient under such circumstances will fall down perhaps in a fit from want of circulation in one side of the "circle of Willis," formed by these arteries; yet as the cause is so apparent, the danger may not be very alarming. Some instances of cure of aneurism of even the innominate have been given by American surgeons, in which recourse was had to ligature on Brasdor's plan of the subclavian; the result here ought to be equally dangerous. Hodgson gives us cases also in which a plug of effused lymph had nearly obliterated the subclavian; while Gendrin has imitated all the phenomena of arteritis and blocking up of aneurisms by injecting irritant substances into a portion of artery contained between two ligatures. In Mr. Fergusson's new mode of operation, we believe an entirely novel idea is acted on—namely, the displacement of the lamellated fibrin of the aneurism, on which no operation has been performed, and so directing the clots of fibrin that they shall block up the distal end of the artery so diseased. As Mr. Fergusson has expressed an intention of bringing the entire subject under the notice of the Medico-Chirurgical Society, we purposely abstain from giving the cases in detail. The method of treating aneurism by compression, originating with Desault and Hunter, and recently revived with such excellent results by the Dublin surgeons, will gain an immense accession of interest, if it should prove that the fibrinous deposit of the sac of the aneurism may be thus, as it were, utilized in bringing about the results hitherto gained in a different mode by Brasdor's operation at the distal end of the aneurism. Considerable caution will be at first necessary, as observed by Mr. Fergusson, in selecting cases which are fitted for the present method, as premature or ill judged experiments in the shape of direct pressure or manipulation on the sac of aneurism not requiring it, one of which we mentioned recently as brought into Guy's where direct and prolonged pressure had been made in the popliteal space before the patient came into hospital would be certain to be followed by severe inflammation of the sac and other dangerous results. The spontaneous cure of aneurism is not unknown in practice; it may take place, it must not be forgotten, by a coagulation of the contents or increase of the quantity of lamellated blood in the sac, the cavity becoming filled, and the circulation conveyed to the parts beyond the disease by the collateral vessels; or, again, in some rare cases, the aneurismal tumor may be doubled up and press upon the portion of artery leading directly to the aneurism; or in a third fashion, as in a remarkable case given by Mr. Liston, where the patient had well-marked subclavian aneurism, which subsided and disappeared—an aneurism of the innominate pressing on and obliterating the aneurism of the subclavian!

Whatever may prove to be the correct pathological explanation of the phenomena in Mr. Fergusson's present cases, we deem it our duty to state here briefly that the cure seems complete and unequivocal without any ligature of vessels,

nor is there any reason to believe the case was one of spontaneous cure of subclavian aneurism, as in the case given by Mr. Liston. It is now two years since the man came first under observation; he has been, on and off, under treatment all that time in King's College Hospital and at home in the country; but happening to be in town within the last fortnight, Mr. Fergusson took advantage of the opportunity to exhibit the case to his class.—*Lancet*.

**POISONING BY OXALIC ACID.**—The patient was a lad, sixteen years of age, who took, according to his own account afterwards, a drachm of the acid between seven and eight in the evening. In about an hour afterwards he was found lying in a lane, moaning, and with some colored fluid (as if from vomiting), close by him. He was seen by a surgeon who was passing; he was insensible, pulseless, and his lower jaw was spasmodically closed. Cold water dashed on his face restored sensibility, and relaxed his jaw. The boy said he had eaten the acid without dissolving it in anything. He was removed home in a cart some distance, and Mr. F. Clarke, of Luton, saw him between ten and eleven. He found him but partially sensible, very drowsy, and in a state of collapse; he had vomited a bloody matter, as well as some whitish fluid, which did not contain any crystals; his tongue and lips were unusually pale, but there was no excoriation; he died about half-past three the next morning. After death the tongue was found dotted over with white specks, the œsophagus was not inflamed; the stomach was extensively disorganized, and had the appearance of gangrene in some parts. Portions of the mucous membrane were detached, exposing the muscular coat. The points of interest in the case were, the youthfulness of the victim, the taking the poison in a solid state, and the small quantity of it which proved fatal. The severe lesion of the stomach was probably owing to the solid and concentrated condition in which the poison was introduced into it.

Dr. Snow related a case of poisoning by oxalic acid in a young woman aged seventeen. The poison was partially undissolved, and she died rapidly. After death, the mouth and lips were found very white, the stomach was black and congested.

Dr. Richardson believed that the smallest dose of oxalic acid ever known to produce death, previous to this case, was four drachms. He related a case in which six drms. were taken by a young woman; spontaneous vomiting occurred, and she recovered, but suffered afterwards from salivation and a spongy state of the gums. In another case in which an ounce was taken, insensibility and coldness followed; but the patient recovered under treatment. There was little difference in effect, in regard to quantity, between oxalic and citric acid, the former of which might be prescribed, he thought, in larger doses than was usually done.

Dr. Routh and Dr. Rogers observed that the post-mortem appearances observed in Dr. Barker's case were such as commonly presented themselves in cases of poisoning by oxalic acid.

Dr. Crisp remarked that the case was defective, in the absence of any statement as to whether any antidotes were applied. If such was given, the poisoning by so small a dose was the more remarkable.

Mr. Edenborough observed that there was no evidence, except that of the boy himself, as to the quantity of the poison taken. What became of the three drachms over the dose?—*Lancet*.

**DEATH FROM INHALATION OF CHLOROFORM IN EDINBURGH.**—As Edinburgh has so long enjoyed an almost complete immunity from accidents resulting from the use of chloroform, the following case, which is reported in the *Edinburgh Medical Journal*, is worthy of attention. A lady, aged 36 years, called on Dr. W. A. Roberts, in order to have some teeth extracted. As she had inhaled chloroform once, during an accouchement, and as Dr. R. had also administered it to her on four previous occasions during the past year, he consented to employ it. She had only taken about nine or ten inspirations, when, in less than a minute from the time she began to inhale, and while speaking, she gave a convulsive start, and with a stertorous inspiration, and the eyes and mouth wide open, sunk to the

floor. Dr. Simpson, being near at hand, was sent for, and arrived in less than five minutes, with Dr. Priestley. The means employed for relief were artificial respiration, galvanism, and bleeding, though only a few ounces of blood could be obtained. After artificial respiration had been carried on for some time, spontaneous inspirations took place, the pulse became distinct, and the lividity of the face in a great measure disappeared. But these favorable indications ultimately declined, and after one hour and a quarter of the most energetic exertions (especially on the part of Dr. Simpson), the case was reluctantly abandoned as hopeless, life being manifestly extinct.

At the post-mortem examination the chief morbid appearances were found in the heart. This organ was very small, the right side flaccid and full of blood, the left firm and contracted. The walls of the right side were unusually thin, and their tissue was soft and lacerable. Under the microscope, the muscular fibres of the right ventricle were much altered in appearance; the transverse striæ were indistinct, or had entirely disappeared in some portions, while fatty granules were everywhere observable, arranged in lines along the direction of the fibres.

The father of the patient had died of disease of the heart, being found dead in his chair.—*Boston Medical and Surgical Journal.*

DEATH FROM CHLOROFORM.—In our last number we noticed a case in which death occurred from the inhalation of chloroform recently in Edinburgh. We regret to state that the same accident took place in this city on Saturday, Jan. 5th. We copy from the Boston Journal the following statement of the case, as prepared by Dr. Emmery, who administered the chloroform.

"Between the hours of 1 and 2 o'clock on the 5th inst., I commenced to administer chloroform to Mrs. P. A. Morgan, at her request, for the purpose of removing some teeth. I commenced with a small quantity—should think from two to three drachms, on a sponge. She inhaled it without difficulty for a minute or two. Her pulse was not strong, but uniform. She then commenced to be excited, and said that I was going to extract her teeth, and she should know all about it. She said that Mrs. Paige (the lady who accompanied her) was getting the forceps to extract them with. I think about one minute had passed during this conversation and excitement. I then removed the sponge from her mouth, and in a few moments she became quiet, and satisfied that there had been no attempt made to remove her teeth. In a few moments I commenced the operation again with the same amount of chloroform. She inhaled it without difficulty, about as long as she did before, and became so much excited that she got up out of the chair and insisted that I had extracted her teeth. She spit upon the floor and looked to see if it was blood, and she insisted that some one was coming into the room whom she did not want to see. I sat her down in the chair again, and she then went into a spasm, closed her teeth, and breathed with difficulty. I sprinkled water on her face, and the muscles relaxed, and I asked her to get up and we would place her on the lounge. She made an effort to rise, and with my assistance stood on her feet, and then instantly sank to the floor. With the assistance of Mrs. Paige, I placed her on the lounge, and there was a rush of blood to the brain. I sprinkled water in her face again, but she showed no signs of being conscious. Mrs. Paige went for assistance, and I immediately commenced artificial respiration by insufflation, and kept it up until Dr. Stedman came in, which was but a few minutes." To this account by Dr. E., the Journal adds—

"As was stated in our paper yesterday, the inquest was held by Dr. C. H. Stedman, and the jury returned the verdict "that the deceased came to her death from the effects of the chloroform, and that the chloroform was a pure article, and was given at the urgent solicitation of the deceased, and with all proper care and discretion." They further say, "from the testimony and opinion of medical experts in this case, the jury feel compelled to caution the public against the use of chloroform, as being a dangerous anæsthetic agent."

With this recommendation we entirely agree, and we have before urged, not the necessity of caution (for caution seems to be of no avail in these cases,) but the abandonment of chloroform and concentrated chloric ether, as anæsthetic



agents, in ordinary cases; and more especially since we have the original article used for producing insensibility to pain, sulphuric ether, which is efficient, cheap, and above all, safe. We are not aware that any case of death has occurred from the direct effect of the inhalation of ether, and although it is possible that such an event may take place, the article is beyond all question more safe than chloroform, the number of deaths from which now amounts, we fear, to thousands.

We cannot help thinking that the amount of chloroform used in this case was very large. It appears that from "two to three drachms" were first inhaled, and that the same amount was repeated. We believe that the most approved practice in England is to pour a few drops (twenty minims, Druitt) on a handkerchief folded into a hollow cone, or into an apparatus specially designed for the purpose and held at a distance of a few inches from the patient's nose. This is to be repeated occasionally until anaesthesia is produced; in many cases a single drachm is sufficient.—*Boston Medical and Surgical Journal*.

**EXTRAORDINARY SURGICAL OPERATION—TAPPING THE PERICARDIUM AND INJECTING IT WITH IODINE.**—Among the curiosities of medical experience in Paris, we may notice a remarkable case reported to the Academy of Medicine, at its session of November 6th, by M. Aran, being no less than the rare and bold expedient of tapping the pericardium and injecting it with a solution of iodine, for the cure of pericarditis, and its result—effusion. In presenting the facts of the case, M. Aran proposed to show that the operation which has hitherto been regarded as among the most rash, dangerous and uncertain in surgery, may, with proper precautions, be as safely and easily performed as that of tapping the thorax or abdomen.

*Case.*—A foundryman, of 23 or 24 years of age, of feeble constitution, was, at the end of 1854, the subject of pleurisy of the left side. A month after the termination of that attack he was affected with pain in the region of the third or fourth false rib of the left side, slight oppression, and palpitation of the heart when working. These symptoms spontaneously disappeared during the fine weather. Towards the middle of the succeeding July, he was attacked with "*fièvre céphalique*," extreme lassitude, and above all, with pain below the left nipple, palpitations of the heart and dyspnoea.

July 27th. Symptoms of pericarditis manifest; febrile excitement intense; lancinating pain in front over the fourth and fifth left intercostal space, increased by pressure; acute sensibility to compression in the epigastrium; precordial dulness considerably augmented, extending over a space measuring 12 centimetres\* vertically, and 14 transversely; impulse of the heart very obscure—its sounds feeble and distant. Diarrhoea, which had existed eight days, and a suspicious state of the lungs, forbade the idea of general detraction of blood. Six scarified cups were applied to the affected side—the succeeding day calomel was administered in small doses, and mercurial frictions were applied over the anterior region of the chest with the view of hastening pytalism. Two flying blisters (*vésicatoires volants*) were applied in succession over the precordial region. Notwithstanding these applications, effusion increased, and three days after the pulse was feeble, irregular, unequal, and extremely frequent.

August 7th. Augmentation of the general and local symptoms, suffocation imminent. The extent of the precordial dulness is increased two centimetres transversely, complete obscuration of the sounds and impulse of the heart exists—the liver is displaced from above downwards.

The failure of the various means of relief applied up to this time, induced M. Aran to resort to puncturing the pericardium. Rejecting the operating procedures of MM. Riolan and Larrey, he adopted that erroneously attributed to Senac, consisting in penetrating the cavity of the pericardium, between the fourth and fifth intercostal spaces, with a capillary trocar, rather than, by using the common trocar, risk wounding the heart, and producing mortal hemorrhage. The operator

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\* Centimetre—4.72 English lines.



also adopted the following precautions: By percussion the circumference of the pericardium was circumscribed by a series of concentric lines, starting from different points of the chest, and stopping at the heart, and the space (*forme*) of the dullness designated with care. The attempt was then made to limit the zone within which the silence of the sounds of the heart was complete, that where they began to appear, and that where they were very clearly heard. These sounds, which were completely absent in the inferior portion of the dull part, reappeared, yet feeble and distant, in the fourth intercostal space, leaving then a zone of sufficient extent within which the trocar could be plunged from before backwards, without running the risk of impinging upon the heart. To be fully assured, a point was chosen for the operation in the fifth intercostal space, two or three centimetres from the extreme limit of the dullness, on the level of which an incision was made through the skin with a lancet, and thrusting the trocar in slowly, says M. Aran. I soon arrived (after having once introduced the stylet without seeing any fluid flow) within the pericardium, the jerking flow of the liquid at the instant leaving no doubt in this regard.

850 grammes\* of reddish colored serum were withdrawn by the trocar. The fluid first flowed in jets, then in a stream, the patient aiding its evacuation by prolonged respiratory efforts, so marked was the relief it afforded him. Percussion showed that the line of dullness descended as the fluid flowed, and auscultation allowed the beats of the heart to be perceived more and more clearly, without any friction sound; the pulse itself became more distinct, more regular, and less frequent, descending from 120 per minute to 96.

Sustained by the success of the measure in pleurisy, I practiced with precaution in this case the injection of iodine, streaming into the pericardium a mixture of 50 grammes of tincture of iodine, with 50 grammes of distilled water, and one gramme of iodide of potash. The introduction of this was not even felt by the patient. After retaining it several minutes in the cavity, several grammes of the fluid were permitted to flow out—the wound was then closed by graduated compresses and bandage.

On the 19th of August, twelve days after the first operation, a second was required, which gave issue to 1,350 grammes (more than 42 fluid ounces) of a very albuminous, greenish fluid, resembling bile in color. The injection was now repeated, containing four grammes of iodide of potash, almost the entire of it being left in the pericardium. The efforts made by the patient to aid the evacuation of the morbid fluid, determined the admission of a quantity of air into the serous cavity, as manifested by *bruit de gargouillement*, or *de clapotement*, described by M. Bricheateau, and by tympanitic sonoriety in the precordial region. These phenomena disappeared in a few hours, but the effusion began to re-accumulate on the evening of the day of operation. Up to the 21st of August the dullness increased. On the 22d it remained stationary, and by the end of the 23d it commenced diminishing. Soon the *bruits* began to be perceived, although feeble, at the apex of the heart, and by the 28th of August, the dullness did not extend beyond the median line in bruit, the nipple on the outer side, and the third rib superiorly.

In proportion as the affection appeared to subside on the side of the heart, signs of tuberculosis of the lung became more and more evident, chiefly in the left side, where the signs of inflammation has been first noticed.

Towards the end of September, œdema of the lower extremities was noticed, which did not become general. Since the end of October there has been return of strength and appetite, and amendment of the thoracic symptoms and of the general health. M. Aran expressed the hope that this case, which established the safety of injecting solutions of iodine into the pericardium, would encourage physicians to practice an operation which was called in to save the life of the patient, who without it must inevitably have perished.

The subject of the operation was then presented to the Academy.—*La Gazette Hebdomadaire*.

\* The French is equal to 15.434 grains Troy.

# UNIVERSITY OF LOUISIANA.

## MEDICAL DEPARTMENT.

### THE ANNUAL COURSE OF LECTURES

IN THIS DEPARTMENT WILL COMMENCE

**On MONDAY, NOV'R. 19, 1855.**

AND WILL TERMINATE IN THE ENSUING MARCH.

#### FACULTY.

JAMES JONES, M. D., Professor of Practice of Medicine.  
J. L. RIDDELL, M. D., Professor of Chemistry.  
WARREN STONE, M. D., Professor of Surgery.  
A. H. CENAS, M. D., Professor of Obstetrics.  
A. J. WEDDERBURN, M. D., Professor of Anatomy.  
GUSTAVUS A. NOTT, M. D., Professor of Materia Medica.  
THOMAS HUNT, M. D., Professor of Physiology and Pathology.  
CORNELIUS BEARD, M. D., } Demonstrators of Anatomy.  
SAMUEL CHOPPIN, M. D., }

The rooms for Dissecting will be open on the third Monday in October.  
The faculty are Visiting Physicians and Surgeons of the Charity Hospital, and attend this institution from November to April.

The Students accompany the Professors in their visits, and, free of expense enjoy extraordinary practical advantages.

There are, during the session, about eight hundred persons prescribed for daily.

In 1854, the number of patients was thirteen thousand one hundred and ninety-two.

— The Administrators of the Charity Hospital elect annually, in April, fourteen Resident Students, who are maintained by the Institution.

THOMAS HUNT, M. D., *Dean.*

**DR. C. BEARD, OCULIST,**

No. 5 Carondelet Street, New Orleans.

#### REFERENCES.

DR. WARREN STONE,  
DR. A. H. CENAS,

DR. E. D. FENNER,  
DR. T. MEUX.













